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## ARCHETYPES AND SYMBOLISM[1]

It has been a custom in this association to grant an incoming president three great privileges: he is allowed to address you for more than twelve minutes; he is allowed to speak on any topic he chooses, and, thirdly, what he says is allowed to go without immediate contradiction and embarrassing discussion. Most of your preceding presidents have utilized this attractive opportunity for departing from the troublesome humdrum problems of the day and gaily sailing forth on a sea of generalities where there are no limiting shores or submerged rocks in the way of facts to annoy one. This precedent allures me and I propose to indulge just this once. The particular speculative cruise on which I would have you join me is indicated in the title, and it involves a discussion of a characteristic prevalent among writers and teachers—including teachers of anatomy. As I see it, teachers have a shameless yearning for the diagrammatic; the tendency to state things in a simple way, even when the things themselves are not simple; the tendency to supply sharp contours or classifications where the real margins are indistinct or the parts intimately blended. Apparently the impression is prevalent that there are simple laws and ground plans underlying all that we see about us, and for many years the investigator has been in hot pursuit after them. Our great heroes are those who succeed in cleverly expressing the complex phenomena of nature in the form of precisely stated laws, or archetypal patterns and we grade our heroes according to the length of time their laws or patterns endure. Let us consider the nature of this situation and see what is to be done about it.

If we stop and picture to ourselves some of the circumstances of the beginnings of our guild, the original utility of diagram and symbolism is plainly evident. It is difficult to see how civilization could have developed without them. As you all know, the origins of the medical and biological sciences are to be found in the ancient priestcrafts. Among the earliest civilizations, it was the priests who were the possessors of the truth. They fostered what there was of learning. Much of their information came to them through direct revelation—by virtue of their peculiar and magical understanding of the desires

[1] Presidential address, read before the American Association of Anatomists, at the Nashville meeting, April 14, 1927.

and purposes of the Governor of the Universe. But they also acquired and handed down a considerable amount of information obtained through observation and experiment. They were the first professional teachers. In addition to ministering at the sanctuary they taught the laws of hygienic, social and ethical conduct and explained the nature of the universe as they understood it. The training of new recruits in the craft and more especially the expounding of doctrinal matters to a laity incapable of comprehending the higher ranges of ecclesiastical wisdom involved the pedagogical device of simplification. We thus find in that remarkable heritage of ours, the chronicles of the primitive Jewish tribes, evidence of the great extent to which they relied on analogy, parable and diagrammatic representation. Likewise we find the Egyptian, Oriental and early Christian civilizations rich in symbolism, of which we still have many persistent relics. From those days to this the practice has continued. The chosen ones, those to whom all is revealed, talk down to their benighted contemporaries, in simplified edition.

As time has gone on, however, many things have happened. Oral and written languages have been perfected. Mutation and specialization has occurred among the priests. Various groups have budded off to devote themselves to special intellectual and scientific pursuits and thus a medical profession has come about and finally among the latter we recognize our own group, the anatomists. With this there has been a great increase in observed fact in proportion to revelation and a corresponding gradual decrease in the necessity for symbolic thinking and pedagogy. As observed facts increased better ways of discovering facts were devised and greater zeal shown in the search of them. The curve of the rate of their accumulation bent sharply upward and a new technique was required for their adequate recording and teaching; thus classification came into existence. This resulted in a relative recession of symbolism but not in its complete disappearance. The mass of new facts explained many of the old problems, but they also introduced new riddles of their own. There was still opportunity for revelation. There was still occasion for those gifted with lively vision to talk down, though not so far down as in primitive times, to the less gifted ones.

Coming to modern times one finds more facts than ever before. Our systems of record and classification have steadily advanced. We have invented new techniques of fact-analysis and fact-prediction. We are bristling all over with facts and our craving for them is insatiable.

In the meantime the position of revelation in its raw form has become precarious. It has to go around under cover and we deal with it, as with bootleggers, only as a matter of expediency and where facts fail us. Even its more refined cousin, transcendentalism, has become taboo. It is not until we come to its second cousins—concept, theory, idea and schema—that the family is allowed in good company, and they are only allowed pending good behavior. We require that they remember their place.

The situation remains that a complicated world is to be explained. The explanations must be formulated in terms sufficiently simple for our comprehension, just as was true with primitive man. We, however, can deal with more complicated propositions than he could. The child starts with simple numbers, he then masters fractions and decimals, but it is not until he has advanced to applied mathematics that he can successfully deal with variable and approximate quantities. In the present state of our development, amidst facts there remain many yawning chasms of uncertainty—too great for us to reckon with directly. We require the help of a certain amount of definiteness, simplification and a little touch of revelation. So it comes about that slogans appeal to us as they did to our predecessors. The world is still largely ruled by phrases, oracular utterances and sweeping generalizations. The popular professor talks glibly of archetypes and underlying principles and draws marvellous diagrams of nature's mysteries. The audience cries, "Give us clarity or give us death."

One of the devices commonly employed for securing convincing simplicity is the adoption of numerical headings or subdivisions. In that way a proposition is more easily presented and also more easily grasped. It appeals to our sense of authority and exactness. Moses understood that and our most successful lecturers and textbook writers understand it to-day. We have firstlys, secondlys and thirdlys. We divide objects and phenomena into parts, periods and stages, knowing oftentimes there are no interruptions like those inferred. Every schoolboy learns that "All Gaul is divided into three parts." The great master of descriptive writing who was authority for this failed to mention the province which was, in fact, a very lively part of Gaul and he also completely disregarded Cisalpine Gaul. Furthermore, he subsequently informs the reader that a great many different tribes inhabited Gaul. In spite of our realization of its artificiality and misleading nature the concept of a tripartite Gaul stays with most of us throughout life.

Philosophers differ as to the most perfect number. Various numbers have had their advocates. Some numbers find particular favor with certain objects. If the digits of an animal do not occur in fives we feel that something is wrong and we seek to explain

. The number four has never acquired any particu-r sanctity—it nevertheless has proved very useful. Ve divide many things in quarters, for instance, the oon. We say there are four points to the compass nd that has helped matters considerably, though it oesn't meet the requirements of those who are most ependent on that instrument. Sailors commonly eckon with eight points and surveyors with many ore. There is only one magnetic direction and here are as many other directions as there are pos-ible divisions of the circle.

As for the greatest popularity I rather think the hrees have it. There is something irresistible about hree. "Alle gute Dingen sind drei." It seems to eet all artistic, physical and intellectual require-ents. It can be recommended to any one contem-lating classification in any field. We say that sub-tances are either animal, vegetable or mineral. Things are good, bad or indifferent. Our education s based on the three R's. Our flag is red, white and lue. Liberty, equality and fraternity won a revo-ution. Rum, Romanism and rebellion defeated Blaine. There are just exactly three graces and no ne has dared spoil the charm by adding a fourth. There are three great desiderata, the Good, the True nd the Beautiful. There are three germ layers—nd so on. You can easily recall hundreds of such rilogies that have enjoyed successful careers.

All this concerns us, for we have often been in-fluenced in our scientific reasoning, and even still are, by such factors as revelation and transcendental-ism, our proneness to clever dicta and our great craving for the simple and diagrammatic and espe-cially for "threes." We forget that these things are merely temporary fallible expedients and confuse their products with verifiable truths. I should like to illustrate this by referring to three typical concepts as instances in which anatomical thought has been influenced and misled in this manner. The first hor-rible example that I am laying before you was sav-agely guillotined by our fathers, the second is already in the electric chair of the present and the third remains to be mercifully chloroformed in the future.

One could scarcely select a more classical illustra-tion of the kind of speculation with which this paper is concerned than the *archetype vertebra* of Owen. The concept of the vertebral nature of the head did not originate with him but had been touched on by a number of his predecessors. Chief of these was the German naturalist, Lorenz Oken. The analogy between the skull and vertebral column was utilized by Oken as an illustration of the mystical system of the "all-in-all" and "all-in-every-part." According to him, "The head is a repetition of the whole trunk with all its systems; the brain is the spinal cord; the cranium is the vertebral column; the mouth is intes-tine and abdomen; the nose is the lungs and thorax; the jaws are the limbs; and the teeth the claws or nails." With regard to the origin of this grand idea, Oken narrates that while walking one autumn day in 1806 in the Harz forest he stumbled upon the blanched skull of a deer, picked up the partially disarticulated bones and on contemplating them the truth flashed across his mind, and he exclaimed, "it is a vertebral column." It is to be observed that Oken had higher aims than to busy himself with de-tailed homologies—he preferred bigger and more gen-eral concepts. He writes:

> A vesicle ossifies and it is a vertebra. A vesicle elon-gates into a tube, becomes jointed, ossifies, and it is a vertebral column. The tube gives off (according to laws) blind lateral canals; they ossify, and it is a trunk skeleton. This skeleton repeats itself at the two poles, each pole repeats itself in the other, and they are head and pelvis. The skeleton is only a developed, ramified, repeated vertebra; and a vertebra is the pre-formed germ of the skeleton. The entire man is only a ver-tebra.

This way of looking at things might simplify the task of the teacher of anatomy, but, unfortunately, none of the statements is true. A vertebra is not an ossified vesicle, as Huxley in his time pointed out, and the vertebral column is not derived from a tube, nor are the head and pelvis repetitions of one an-other. Furthermore, to say that the human body is a vertebra is, of course, ridiculous. It behooves us to remember, however, that intellectual people of the rank of Oken and Goethe at one time discussed this matter seriously, just as intellectual people at an earlier period discussed the question as to how many angels can stand on the tip of a needle—and it may well happen that things we now take seriously, such as genes and recessives, at some future time may in their turn prove ridiculous.

In spite of its extravagant metaphorical specula-tions Oken's vertebral theory of the skull obtained a considerable credence among the ikonophiles. Its vogue might have been more extensive had it not been for the counteracting influence of Cuvier, who attacked it bitterly with facts and sarcasm and who is credited with having driven it out of scientific circles. But hardly had it been thus disposed of when back it came dressed in new raiment by no other than the British Cuvier, Richard Owen, a com-parative anatomist and paleontologist of the first rank. In Owen's "Archetype and Homologies of the Vertebrate Skeleton" we find the idea elaborated in its perfected and completed form.

Owen conceived an ideal typical vertebra, or arche-type, having elements and parts from which all bony

structures could be derived—similar to the electric signs which have bulbs so arranged that illumination of the various combinations can produce any desired letter. He defined a vertebra as one of those segments of the endo-skeleton which constitute the axis of the body, and the protecting canals of the nervous and vascular trunks. The vertebrate frame, in turn, consists of a series of fundamentally identical segments, each being modified to a greater or less degree according to its position and function. The bones of the head compose a series of four vertebrae, essentially similar to those of which the rest of the skeleton is constituted. They are, namely, the occipital, parietal, frontal and nasal, and they each have a neural and a haemal arch, and may or may not have diverging appendages. The arms are appendages of the haemal arch of the occipital vertebra and thus are parts of the head. In a similar way the pterygoid and zygoma are appendages of the most oral of the cranial vertebrae.

In the repetition of similar segments in a vertebral column and in the repetition of similar elements in a vertebral segment Owen saw an analogy to the growth of crystals. He points out that the principle of vegetative repetition prevails more and more as we descend in the scale of animal life, and at the same time the forms of the repeated parts of the skeleton approach more and more to geometrical figures. Furthermore, the calcifying salt in low-organized skeletons assumes the same crystalline figures which characterize it when deposited outside of the organized body. From this he argues that in the development of the animal body there is the concurrence of a general and all-pervading polarizing force like that which produces crystals along with an adaptive or special organizing force.

To Owen the archetype vertebra was a very real thing—an all-pervading polarizing force; to us it is mostly a product of his imagination. Even in Owen's time embryology was already revealing that segmentation of the body is not its primary condition, and it was soon found that when segmentation occurs it does not extend throughout the whole head, and, furthermore, that bones are not all similar in their mode of origin, nor, in this respect, are they the same in the skull as in the spinal column. After Owen's archetypal concept had been given decent burial its executioners, with due piety, granted that there was a grain of truth in it—that it did serve to call attention to the continuity in structural design beween the occipital and upper cervical regions.

The second concept that I would bring to your attention took form and became universally accepted during the golden age of anatomy in Germany. It is chiefly associated with the names of Rathke and v. Baer. Both of these investigators became very much excited when they found the presence of gills (as they first called them) in mammalian embryos—even human. On studying the blood vessels coursing through them, they found a series of aortic arches on each side, similar to what had already been observed in the chick. All this seemed to signify the existence of a transitory branchial apparatus throughout the mammals, and, pyramiding theory upon theory, it later came to be one of the favorite arguments for the recapitulation theory.

In his 1827 paper on the origin and fate of the aortic arches v. Baer gives a surprisingly accurate and discerning account of their development, not being handicapped by the rigid symbolic concept that was to take form and dominate his followers. He notes the gradual appearance of the vascular arches from the front toward the back and correctly states that at no time are they simultaneously active. In another place he points out that migration of the heart results in changes in the direction of the blood stream, and these in turn lead to changes in form of the arches. Some of the arches are obliterated early, some last longer and some remain as permanent vessels. Though realizing the difficulty of expressing the various phases of this phenomenon in a single sketch, he nevertheless attempted it, just as any good teacher would do, and that sketch, appearing in Burdach's curious six-volume work on physiology, is the underlying diagram for all the schemata of the aortic arches which have since appeared in numberless modifications. The first one to be dissatisfied with it was v. Baer himself. So he modified it a little and reprinted it in his comparative embryology. The next improver was his amiable competitor, Rathke, who showed, in a beautifully executed series of drawings, the real ground plan of the aortic arches—the familiar symmetrical gridiron schema, all smooth and uniform and completely stripped of environmental structures. Along beside the type-gridiron are other gridirons representing the plan for reptiles, birds and mammals with various parts colored in red indicating the portions that are permanently retained.

The influence of Rathke's diagram can be seen in most of the diagrams of the long list of improvers that have followed him. Every writer dealing with the subject seems to have had a personal opinion as to how the red should be allocated in the framework, and there has been much uncertainty as to whether six or five is the perfect number of arches. In them all, however, there is the concept of a ground plan for all air-breathing vertebrates, a common fish-like aortic arch pattern, supposed to exist more or less perfectly in the embryo, but surviving only in a fragmentary manner in the adult. The inference is that

blood vessels have a definite individuality in pattern, over and above environmental circumstances, that, of their own initiative, the various vessels strive to attain a particular form. One could imagine, for instance, that if the tiny bit of embryonic endothelium, constituting the primordium of the brachial artery, were transplanted to some perfect culture medium it would result in a main trunk growing out and branches and communications would be given off, one after another, until the usual pattern for the arm and hand became elaborated, and there in plain view would be our old friend, the dorsal interosseous, and all the rest of them. I can imagine the experiment but I do not imagine it would come out that way—and you probably do not, either.

We have learned that of all the structures of the body the vascular system is one of the most adaptive. No organ in its development and maintenance is altogether independent of its environment, but some are more so than others. Some are individualistic and domineering, others are subservient and plastic. Endothelium belongs in the latter group. It is highly responsive to its environment. Thus, when one studies the development of the blood vessels of the brain it is found that they do not independently unfold into the adult pattern but react continuously in a most sensitive way to the factors of their environment. Arteries change to veins, veins to arteries, capillaries to large vessels and large vessels to capillaries with a constant surrender of old channels for new ones. The apparatus is continuously adequate and complete for the growing brain as it exists at any particular stage; as the environmental structures progressively change, the vascular apparatus also changes and thereby is always adapted to the new conditions. The final pattern is the result of the sum of the environmental influences experienced. In short, embryonic blood vessels have no ground plan of their own, independent of the structures around them.

As for the aortic arches, we have learned in recent years that they are and behave much like other blood vessels. To say that they are a transitory set of symmetrical and uniform tubes, a symbol of a phase in ancestral history is no longer an adequate description of them. It has been shown by F. T. Lewis, Bremer and Evans that they are not simple tubes but possess many interesting irregularities in conformance to the structures around them, and that they develop from capillary nets, and revert to capillary nets, and are continuously associated with capillary nets, just like other embryonic vessels and quite apart from any specific gill characteristic.

With this better knowledge of the exact anatomy of the aortic arches it has become apparent that at no time does Rathke's embryonic type really exist in the embryo—any more than does the Owen archetype vertebra. Apparently von Baer appreciated this, but his followers did not. They accepted irregular capillary vessels as vestiges of the arches, therefore virtually arches, and so constructed in their imagination and embryonic type, which at the best was only a composite of several stages.

Nor are the arches or parts of them eradicated bodily, as with a pair of tinner's snips, as indicated in the diagrams. The studies of Heuser in the pig and of Congdon in man have shown us that in the transformation of the vessels of this region probably no endothelium is lost. It all appears to be negotiated through the remarkable capacity of the endothelial apparatus for reshaping itself. By means of their improved technique these investigators were able to trace the development step by step from the formation of the first arch to stages where the mature pattern could be recognized. They demonstrated that, just as in other parts of the body, there is a sensitive vascular response to environmental needs and conditions, in this case the shifting heart, the changing pharyngeal pouches and branchial bar tissues and the acquisition of arm buds. As these change, the endothelial pattern changes and thereby provides at all times an adequate service.

The entire vascular equipment of the aortic arch region can be explained in terms of this service, and there is nothing left over to be accounted for. There is no endothelium left over with which to make vestigial gill vessels, and one can not base a ground plan on things that do not exist. If we start with a diagram of something that does not exist, an infinite number of improvements can not make it correct. That appears to be the situation in which Rathke's diagram now finds itself.

This brings us to the third illustration of a man-made concept to which I would now direct your attention, the theory of the three brain vesicles.

The battle of Waterloo was not the only thing that happened in 1815. In that year Jacob Henle, Charles Darwin and our own Oliver Wendell Holmes became six years old and presumably started off to their respective schools with their mothers' blessings and unexpressed apprehensions. In the autumn of that year a young Dr. Baer from Dorpat matriculated under Professor Döllinger in Würzburg and there received the divine spark that was to make him one of our greatest embryologists. This is the same Dr. Baer to whom I have already had occasion to refer. It was during this same year that Johann Friedrich Meckel, the younger, inaugurated Meckel's Archiv. This latter event immediately concerns us. The first paper of Meckel's Archiv is by Meckel himself and

contains an account of the development of the central nervous system of mammals. He describes, and pictures, the brain of a 6 mm rabbit embryo as consisting of a series of vesicles. Concerning the significance of these vesicles he cautiously states that the most caudal one probably becomes the medulla oblongata; the next, the brain peduncles; the third the corpora quadrigemina; and the irregularly shaped anterior end of the neural tube he ascribes to the brain ganglion and hemispheres.

Malpighi long before had portrayed in his beautiful plates vesicles in the neural tube of the chick, and various observers had referred to the widened portions of the neural tube as brain cells or brain vesicles. But before Meckel no one had studied them particularly nor had traced them to adult structures in mammals as well as birds. The vesicular origin of the brain is formulated by Meckel in a crude but in a sufficiently definite way to be recognized as the forerunner of the concept.

If it is conceded that Meckel was the precursor in the establishment of the three brain-vesicle idea, then we must honor von Baer as the one who some ten years later shaped it into its present form. In his masterly presentation of the development of the chick one finds a detailed description of the widenings and constrictions of the forward end of the neural tube and the consequent formation of three primary brain vesicles; the anterior becoming the cerebrum; the posterior, the cerebellum with medulla oblongata; and the middle vesicle, the quadrigeminal mass with the corresponding part of the brain stem. He further describes that this trinity is a transitory state due to the fact that some of the boundaries are a little slower in making their appearance. Very soon the anterior and posterior vesicles become subdivided each into two parts, making five vesicles in all. These he names: forebrain, twixtbrain, midbrain, hindbrain and afterbrain. It is from these five morphological elements that the brain is finally built—just as we have all been taught and just as we are commonly teaching to-day.

The finishing touch to the conception of three brain vesicles was added later by von Baer's ardent admirer, Theodor L. W. von Bischoff, who illustrated them in his drawings of rabbit embryos. Here we find the source of the precise and diagrammatic interpretation that has predominated in most of the subsequent descriptions of the developing central nervous system.

Since these great pioneers conceived the idea of the vesicular origin of the brain much has happened in the way of method, material and experience, and it is well to stop and inquire: Are the facts bearing out the original conception? Is it all so beautifully simple? Does our brain really develop from three vesicles? Does it pass through a preliminary geometrical state, a fundamental pattern common to all vertebrates, and from which the various parts of the brain are separately derived, in forms characteristic for the species concerned?

We can better understand how the brain-vesicle idea has become so thoroughly intrenched if we recall to what a large extent embryological studies have been based on chick embryos. Now it happens that the chick has one good feature, namely, the availability of the material. The eggs are easily procured, all the year round, and the living embryos can be freely observed at any desired stage of development. It has another feature that is not so good. From the viewpoint of those primarily concerned with mammals, the chick is a very special and peculiar animal and its developmental processes can be interpreted in mammalian terms only with careful reservations. It has been the misfortune of embryology that the embryo most easily obtained, and the one that has been most studied, is, because of its remoteness from the mammalian stem, about the poorest one that could have been selected. Our minds have been saturated with impressions obtained from chick material and many of our embryological troubles are to be attributed to interpretations based on this eccentric form.

It is so with the brain vesicles. It happens that the bird has huge paired optic lobes as a visual center and terminal nucleus for its large optic nerves. They overlie the entire midbrain region as a prominent rounded eminence, entirely concealing the acoustic centers and the other tegmental structures. There is presented an entirely different picture from what is seen in mammals, where the optic colliculi are scarcely larger than the acoustic colliculi. Knowing this, one is prepared for the prominent widening of the neural tube in the midbrain region of the chick embryo. What one actually sees is the large vesicular primordium of the optic lobes, the other tegmental structures being lost in the adjoining parts of the neural tube. The so-called midbrain vesicle of the chick should not be considered as an indifferent segment of the neural tube, resembling, but diagrammatically set off from, the brain segments in front and back of it. Instead, what one sees are the definitive optic lobes, characteristic from the outset and evidently predetermined in structure long before the closure of the neural tube. These optic lobes are not identical with what is ordinarily included under the term "midbrain," although in the chick they form a large part of it. If we are to be accurate, it is therefore incorrect to speak of them as the midbrain

vesicle. This point was not appreciated by the early embryologists.

So long as the chick appeared to have such a handsome midbrain vesicle, their scientific conscience did not prevent embryologists from accepting the irregular portion of the neural tube in front as the forebrain vesicle, even though it scarcely conformed to the term "vesicle." Nor could the hindbrain vesicle have obtained recognition except through the prestige of its midbrain neighbor. Moreover, the general concept appealed to them. The simplicity and diagrammatic possibility of three rather equivalent symmetrical brain vesicles—a temporary fundamental archetypal state shared by all vertebrates, like a well-rhymed proverb, argued strongly for its truth. It seemed to be a golden key that could unlock the secrets of that bewildering structure—the brain. Is it any wonder that the idea made such a deep impression on those who based their embryology on the chick and who studied at a time when the detailed anatomy of the central nervous system was none too well known?

Coming to mammalian embryos where there are no large optic lobes to masquerade as a midbrain vesicle, it became necessary for the embryologists to find a new one and this was not an easy thing to do, because there does not appear to be any. Characteristic of such dilemmas there came to be several opinions as to just where the midbrain vesicle was located. The number of opinions was about the same as the number of investigators. Bischoff solved the difficulty by putting a bird-like nervous system in a rabbit embryo. He was so deeply imbued with the concept that he could see nothing else. It should be said, however, that the investigators of the preceding generation had very little early mammalian material at their disposal. Their acquaintance with the brain did not become intimate until after the closure of the anterior neuropore, by which time the supposed three brain-vesicle period is over and the permanent nuclear masses are already defined. They realized that what they saw were not the vesicles but traces left by the vesicles, that is, they so interpreted them. It was a relatively easy matter to select certain constrictions of the brain lumen, subdividing it into the desired three parts. The more convincing of these was the constriction that eventually becomes the Sylvian aqueduct. The one separating the anterior and middle vesicle was not so good, but it answered the purpose of seeming to substantiate the idea. The real jolt came when we began to get embryos of still earlier stages and these constrictions disappeared without our being able to trace them back as boundaries of the original vesicles, proving that they are not primary but secondary growth phenomena belonging to the older stages. Before going any further I hasten to say that I can see no harm in dividing the brain into a forebrain, midbrain and hindbrain, even if the brain vesicles do not exist—at least until we find some better way of disjointing it.

It is only in very recent times that we have obtained and learned the technique of handling embryos sufficiently young to reveal these early steps in development. With this advance the name of Bartelmez is to be associated. In identifying the precocious début of the primordium of the equilibration apparatus he has given us a landmark of the greatest possible value. He has shown us that this primitive special sense organ is laid down, and the corresponding portion of the brain wall definitely determined before the lips of the neural groove have come together, before there could be any possibility of a brain vesicle.

In other words the brain begins to build its definitive parts before the closure of the neural tube—without going through a preliminary archetypal indifferent three-vesicle stage. With further experience and additional material this has been abundantly substantiated in the pig as well as in man. There seems to be no evidence that the brain wastes any empty gestures toward the past. With no false moves it proceeds directly with the building of an organ appropriate in all its parts for the respective species. It has taken us a long time to find this out. It probably would not have taken so long if we had not been so well satisfied with the diagrammatic concept of three brain vesicles.

Is it to be understood from what has thus far been said that the author of these lines believes that simplification and diagram have done us more harm than good and should be cast ruthlessly overboard? Bless me, no! Quite the contrary! One could write just as well outlining the great services they have performed. We all know very well that advance in knowledge has come about, first of all, through separating the known facts from the surrounding haze of ignorance. These known facts then have been made more significant through classification and coordination, and finally it is through the device of schemata, types and hypotheses that we have been guided in the acquisition of still more facts. The point I am making is not a new one, but it bears frequent repetition. Facts are desirable possessions, so are theories, but the two should not be confused. Facts should be kept in one pocket and theories in another. One should never forget that diagram, classification, symbolism, and hypothesis are but temporary expedients. They are good servants but poor masters. Like all man-made things they are imperfect, and as new facts come into view they must be

revised or discarded. Moreover, those of us who are prudent will be particularly wary of the quick and simple explanation of the processes of living matter. In his classical monograph on comparative embryology von Baer places on the back of his title page the Latin slogan: "simplex est sigillum veritatis!"—simplicity is the seal of truth. That may have been a good working hypothesis at the time; but in view of our new knowledge of the remarkable intricacy of nature should we not change it to read:

Complex est sigillum veritatis!

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## HOW THE TAXONOMISTS MAY UTILIZE THE INTERNATIONAL COMMITTEE ON NOMENCLATURE[1]

Stability in botanical nomenclature has been sought sporadically ever since there have been recognized systems for naming plants. At Paris in 1867 an international congress of botanists formulated a code called the "Laws of Botanical Nomenclature." About forty years later another international congress drew up another code, the "International Rules of Botanical Nomenclature," based in part on the Paris code, but introducing many alterations. A third congress held at Brussels in 1910 amended and enlarged these rules somewhat but did not materially modify them. It was proposed to hold these international congresses at five-year intervals, the succeeding one to be at London in 1915, but the World War interfered and the London congress did not meet. A fourth congress was interpolated at Ithaca in 1926, but no regulatory legislation was adopted; and the fifth congress was authorized to be held at London in 1930 at which the international rules will be again considered.

The Ithaca congress made an important contribution to the history of botanical nomenclature by appointing an international interim committee on nomenclature[2] to consider proposals for amending the international rules. Botanists now have a method by which amendments may be brought before a large committee for adequate study in advance of the congress. It is important that legislation adopted at a congress should be based on facts and should represent a real consensus of the botanical opinion of the world. If taxonomists take sufficient interest in nomenclature to present their ideas to this committee and to support their opinions with carefully prepared arguments and with sufficient evidence, the congress can legislate upon the basis of a fairly accurate knowledge of the actual taxonomic opinion.

During the last decade of the last century, and several years before the Vienna Congress, a group of American botanists formulated a carefully thought-out series of rules of botanical nomenclature, which has been known as the American code. It was felt by these botanists that the nomenclature then in use, based in part on the old Paris code, was in many respects illogical, and gave little promise of ultimate stability. In the American code all compromises, exceptions and concessions were thrust aside and a series of rules was built upon a foundation of principles, the chief of which were the type concept for the application of names and the strict acceptance of the principles of priority (dating from 1753) in establishing the validity of names.

It was thought that the advantages of such a code would be so evident that it would be accepted by the botanical world as soon as the rules were understood. The application of the American code to nomenclature of the day would result in the replacement of many well-known generic names, but it was thought that, the initial changes having been made, the names would not be subject to further change. I accepted the American code with enthusiasm and I have followed its provisions for thirty years. My experience during these years leads me to state that the American code is a good code, easy to apply and definite in its application. If we had built our nomenclature on such a code from the beginning it would now be as stable as any nomenclature could be. If all the world would adopt the American code we would reach ultimate stability in the same degree. In 1918 and the following years a committee of the Botanical Society of America prepared the "Type-basis Code of Botanical Nomenclature.[3] This is a modification of the American code in which the rules for typification are amplified and made more flexible, and certain provisions eliminated which experience had shown to be inexpedient.

At present the botanical world is divided in its support of the two codes, the international rules and the American code. With few exceptions the botanists outside of the United States support the international rules. In the United States approximately half the taxonomists are following the American code. The supporters of the international rules do not all follow the detailed provisions of these rules, but

[1] Read at the Philadelphia meeting of the Botanical Society of America.

[2] See Science 64: 290–291, 1926.

[3] Science 49: 333–336, 1919; 53: 312–314, 1921; the complete code is found in Hitchcock, "Methods of Descriptive Systematic Botany," 201–206, 1925.

they align themselves on the side of the rules as against the American code.

It is evident that the best interests of taxonomy are not served by perpetuating indefinitely two independent codes. The botanical public, without a clear realization of the difficulties, press the taxonomists for an agreement. Absolute agreement can scarcely be expected, as that has not been attained among any body of scientific workers. It is possible, however, to have what might be called a practical agreement, especially as to the use of generic names.

Probably the chief objection to the American code has been to the replacement of well-known generic names under its provisions. The underlying wishes of those who formulated the international rules was to preserve well-established names. It was difficult to do this by a series of rules because the ultimate effect of the rules could not be foreseen in all cases. The adoption of a list of Nomina Conservanda was a clear evidence of the desire to crystallize general usage, although the list itself was hastily prepared and was not the result of careful investigation.

Botanists should understand that a strict adherence to the international rules has resulted in many replacements of well-known names, and such changes will continue. If it be considered an objection that the adjustments to a code result in a change of names, then that objection will apply to the international rules as well as to the American code, though probably not to the same degree.

In my opinion taxonomists would do well to retain well-known generic names, regardless of the restrictions concerning priority, synonyms and homonyms, in so far as these names concern important economic plants or genera with large numbers of species.

I have investigated the names of the grass genera as to their differences under the international rules and under the American code. The technical notes will be published elsewhere, but some of the results may be of interest here. The differences in the names of genera as accepted under the two codes come under four divisions: (a) Names on the list of Nomina Conservanda (*e.g.*, Glyceria vs. Panicularia); (b) differences due to homonyms (*e.g.*, Setaria *vs.* Chaetochloa); (c) differences due to typifications (*e.g.*, Sorghum *vs.* Holcus); (d) differences due to mistaken identification (*e.g.*, Eatonia *vs.* Sphenopholis).

Several of the generic names rejected in the list of Nomina Conservanda have been accepted under the American code but are ineffectively published under the type-basis code. A few ought to be removed from the list of Nomina Conservanda, as there was no adequate reason for conserving them (*e.g.*, Ctenium *vs.* Campulosus). Several ought to be added to the list in order to conserve names sanctioned by usage (*e.g.*, Sorghum *vs.* Blumenbachia).

### Amending the International Rules by Changes in the List of Nomina Conservanda

One of the British proposals was that the list of conserved names should be revised. I am strongly in favor of this, but I fear it is impracticable and inadvisable to have it all done at one time. There is no person or group that has the time, the inclination or the taxonomic knowledge to perform the task. It is entirely practicable, however, to revise the list little by little as various groups are worked over taxonomically. The study of names apart from the organisms they represent should be discouraged.

When an author revises a group he may find that nomenclatural changes are desirable and, for the most part, will make these in accord with the rules. But proposed editions to or eliminations from the lists of Nomina Conservanda may be presented to the International Committee on Nomenclature with the supporting evidence. The committee after consideration will submit a recommendation to the succeeding congress which will make the final decision, presumably validating proposals that have received the support of the committee.

I believe that we can eventually make this list one that will be accepted by the great majority of taxonomists, because each name will have been considered on its merits. Much critical investigation must be carried out before this is done. The weak point of all codes is that they are, in a way, premature; they attempt to establish rules to govern procedure in unforeseen circumstances. A code, like any other human instrument, should be subject to alteration on the basis of experience. The International Committee on Nomenclature gives us machinery by which we can gradually build up a code that most botanists will be willing to follow.

So far as concerns the list of conserved names, a specialist should scrutinize the generic names in the group he is studying. He may ask to have names removed from the list if there has been insufficient reason for conserving them; or he may ask to have names added to the list if it is found that well-established names are in danger of being replaced on the ground of priority. The followers of the American code may find that many of the names they have been using should be rejected on the basis of inadequate publication. It would be well to adopt the policy of avoiding the replacement of a well-established name that is inadequately published, especially if it has been but little used. The presence on the list of names that are valid does no harm, though

such names extend the list unnecessarily. However, it may be advisable to retain many of these superfluous names because the uninformed might not otherwise accept the validity.

The international rules were framed with the intention of preserving well-established generic names. I think it would be desirable to take into consideration this policy in future modification of the list of Nomina Conservanda or in other decisions on conserving names. If it is found that a well-known generic name should, under the rules, be replaced by an earlier name which never came into general use, it would be well, in case there is adequate reason for conserving it, to ask to have this done, rather than to take up the earlier name in publication and make new binomials.

In considering what names should be retained on the list or added to the list by virtue of direct conservation as opposed to the rejection of the alternatives, I would suggest as a working basis that generic names may be conserved against the action of priority if (a) they contain a large number of species, or (b) they contain important economic species, or (c) when the acceptance of an earlier name would invalidate an established homonym in another group.[4] Each case must be decided on its merits by the committee on the basis of the evidence submitted by the botanist asking for the conservation.

### Conservation of Names by Validating Later Homonyms and by Decisions on Standard Species

This method of conservation is not now provided for in the international rules, but I think it would greatly favor ultimate stability in the use of generic names if a method for such conservation were incorporated therein. The present list of Nomina Conservanda conserves later synonyms. It would be well to establish a second list in which later homonyms are conserved. For example, Setaria (Beauv. 1812) came into general use for a genus of grasses. Under the American code the name is invalid because of Setaria Achar. (as published by Michaux in 1803), a genus of lichens and was replaced by Chaetochloa (Scribn., 1897). Since Setaria Beauv. includes a large number of species over the warmer regions of the earth, I think the name should be conserved and I should favor having it placed upon the list of conserved homonyms.

Some differences in usage are caused by the application of generic names in different senses, depending on how an original genus was divided. When for taxonomic reasons a genus is divided the generic name should apply to one of the parts. In some cases there has been a difference in this application. Under the American code the determination of the application of a name in such cases is called typification. When a decision is desirable to establish future usage this can conveniently be done by asking an international congress (through the committee on nomenclature) to decide on the type or standard species of a genus. For example, under the American code the type species of the grass genus Holcus is H. sorghum, but the preponderance of usage has been to segregate the group containing H. sorghum as the genus Sorghum, applying Holcus to a different group. I would recommend that *H. lanatus* be chosen as the *standard* species of *Holcus,* thus confirming the historic development instead of the historic type. Such decisions would appear in a third list, which might be called "Accepted Standard Species."

### Incorporation of the Type Method into the Rules

The British have proposed that "the principle of the type-method of applying names should be formally accepted." I am strongly in favor of this, but I realize that the principle is not well understood by all taxonomists. It may be better to put it in the form of a recommendation rather than a rule. I would suggest that the recommendation be added to No. xviii (under Article 39 of the Rules). At the Brussels congress an addition to this recommendation was made to the effect that in the future the types should be indicated for new genera and species. I would insert after this a statement similar to the following: That when an author revises a genus or other group of plants he indicate what he considers the type or standard species of the genus, or, when it can be determined, the type specimen of the species studied.

The method is so reasonable and so definite that I believe it will be generally adopted when understood. The particular rules for establishing the type or standard species of genera can be added later as botanists become more familiar with the method.

The original presentation of the type method in the American code was as Principle 4, "The application of a name is determined by reference to its nomenclatorial type." Later in the code there were rules for selecting the type, some of which were mechanical. The type-basis code introduced more flexibility into the rules for establishing the type, and defined the type species as being the species or one of the species the author of a genus had chiefly in mind. The British have introduced a new factor, which is in conformity with Principle 4, above, namely, the stand-

[4] If Blumenbachia Koel. (1802) were to displace Sorghum Pers. (1805) on the ground of priority, then Blumenbachia Schrad. (1825), an accepted genus of Loasaceae, would be invalidated.

ard species. If the type species selected in accordance with the rules of the type-basis code result in changing the application of the name which it is desired to retain, another of the original species, called the standard species, is chosen, which will retain the name. By the use of the standard species the type method can be incorporated in the International Rules without disturbing other parts.

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## WILLIAM S. VALIANT

THE late William S. Valiant was curator of the Geological Museum at Rutgers College from the year 1903 until his death on March 27, 1927, except for a short interval in 1919, when he retired and removed to Rome, New York. His paramount interest was in the work of the museum which he had learned to love and he returned to New Brunswick and continued actively until the year 1923, when no longer strong enough to attend to his duties.

Born at Rome, New York, in 1846, he was educated in the schools there and later served as a special teacher for classes in natural history in the Free Academy of his native city for many years before going to Rutgers. He possessed unusual keenness of observation and ability, which enabled him, without the advantage of a university training, to excel in his chosen field of mineralogy. An expert collector, and good correspondent, he added to the collections of the museum and was ever ready to give information to visitors, and was widely known among his fellow workers. An outstanding result of his work was the discovery, of fundamental importance to both biology and geology, of Ordovician trilobites with their ventral appendages attached. A chance find in 1884 led him to search at every opportunity and at the end of eight years in 1892 he was rewarded by finding a layer less than half an inch thick in the Utica shales at Rome, New York, in which numerous fossils occurred, including the well-known form of *Triarthrus becki*, with the ventral anatomy beautifully preserved, even including the antennae.

A large amount of the material was collected and studied by the late Professor Charles E. Beecher, of Yale University, who devoted the last ten years of his life mainly to the preparation and study of the significance of the appendages, but died in 1904 before he had prepared his final manuscript. Professor Percy E. Raymond, of Harvard University, who had studied under Professor Beecher's guidance, then took up the work and published the results of their combined efforts in 1920 as a memoir of the Connecticut Academy of Sciences entitled, "The Appendages, Anatomy and Relationships of Trilobites." In a foreword of this memoir, Professor Charles Schuchert points out the importance of the discovery which made possible studies in the evolution of the Crustacea, which bear also on that of most of the Arthropoda, and gives Mr. Valiant the credit due to his untiring efforts to locate the original material.

Mr. Valiant published scientific articles from time to time. In 1896 he read a paper entitled "Appendaged Trilobites" before the New Jersey State Microscopical Society, of which he was a member. The manuscript was published in the Mineral Collector of Volume 10, No. 3. Two articles on the geology of the Rutgers College campus appeared in the Rutgers *Targum* of April and May, 1898. His unpublished records of the history of the museum and the results of his work point to a life of usefulness and devotion to science, which can not be adequately indicated in this brief sketch.

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## SCIENTIFIC EVENTS

### THE ESTABLISHMENT OF AN INTERNATIONAL BUREAU OF METEOROLOGY[1]

AT the sixth session of the International Committee on Intellectual Cooperation, held at Geneva from July 27 to July 29, 1925, the chairman communicated to the committee a proposal submitted by M. van Everdingen, director of the Netherlands Meteorological Observatory and chairman of the International Meteorological Committee (I. M. C.), with regard to the creation of an International Bureau of Meteorology (I. B. M.) (Annex 4 to document C. 445, M. 165, 1925).

After a brief discussion, the committee requested the undersigned to consider, together with M. van Everdingen and several other experts, how the committee might assist in establishing this bureau.

The present report sets out our conclusions:

M. van Everdingen's proposal was defined in a letter which General Delcambre, director of the French Meteorological Service and chairman of a special committee set up by the International Meteorological Committee, addressed officially to the International Institute for Intellectual Cooperation on November 23, 1925.

The International Meteorological Committee is composed of the directors of the meteorological services of thirty countries (including Germany and Austria), who meet once every three years to discuss scientific

[1] Report by the subcommittee appointed at the meeting of the International Committee on Intellectual Cooperation on July 29, 1925, submitted to the committee on July 26, 1926.

problems of international importance—a definition which covers almost every meteorological problem. The members of this committee are not official representatives of their countries, and the committee possesses no financial resources.

After a preliminary interview with M. van Everdingen, it was decided that the sub-committee should co-opt as experts the members of the special committee appointed by the International Meteorological Committee. With a view to achieving a satisfactory result as soon as possible, a meeting of the sub-committee and the experts was held directly after the meeting of the experts to discuss a draft prepared by General Delcambre.

These two meetings took place on March 27 and March 29, 1926, at Paris. At the first meeting, the technical side of the question was discussed and General Delcambre's draft was approved with certain alterations. Finally, the experts drew up a restricted program, on the basis of which a start could be made, and prepared, in the order of their importance, the following list of the various desiderata to be attained:

(*a*) Administration of the archives of the national Meteorological Committees and a secretariat of the committee to maintain relations with international organs interested in meteorology.

(*b*) Bibliography and retrospective international publications (maps of the Northern Hemisphere, experimental balloons, aeronautical climatology).

(*c*) Organization of the ocean meteorological system; assistance in radio-meteorological centralization and preservation of extracts from ships' logs.

The experts estimate that the minimum cost of carrying out points (*a*) and (*b*) of the restricted program would be 100,000 to 150,000 gold francs.

These were the conclusions submitted at the meeting on March 29, at which were present: M. Lorentz, Mme. Curie, M. Einstein, M. Luchaire; the experts Messrs. Delcambre, van Everdingen, Simpson, Carvalho, Brandao, J. Bjerknes, Werhlé, M. Roper, representing the International Commission for Air Navigation (I. C. A. N.), and M. de Vos van Steenwyck, whom the Committee of Experts had co-opted to maintain relations with the International Institute for Intellectual Cooperation. M. Lorentz presided over the meeting.

The chairman proposed that they should not deal with the technical questions, which has been fully discussed at the former meeting. The experts, he pointed out, were unanimous in endorsing the utility of the proposed organization, and it would not therefore be necessary to go into details. It would be sufficient for them to consider the relations to be established between the future Bureau and the League of Nations through the International Committee on Intellectual Cooperation. The moral support of this committee might already be regarded as assured. The most urgent question was that of obtaining the material resources necessary for the creation and working of the International Bureau of Meteorology.

The discussion which followed this statement showed that the International Committee on Intellectual Cooperation might take action in two ways simultaneously:

1. The International Committee on Intellectual Cooperation might recommend the League of Nations to invite governments to accord subsidies to the International Bureau of Meteorology.

2. The International Committee on Intellectual Cooperation might avail itself of the facilities it possessed in the form of the International Institute for Intellectual Cooperation and place at the disposal of the International Bureau of Meteorology provisionally a few rooms in which the Bureau could instal its secretariat and archives. The cost of installation would thus be diminished and the Bureau might begin work almost at once. It would be understood that, as soon as the International Bureau of Meteorology became firmly established and had proved its value, it would have to obtain its own premises and could no longer remain a charge on the Institute, which must be in a position to offer similar hospitality to any other scientific organization created in similar circumstances. The director of the Institute does not see any objection to such an arrangement.

There will be some difficulty, however, in the way of carrying out the first proposal. It would not only be desirable to establish the International Bureau of Meteorology, but the matter is, indeed, an urgent one. Several meteorological undertakings are about to be abandoned owing to lack of means, and this would create gaps which it would be impossible to fill later.

It therefore seems essential that the International Committee on Intellectual Cooperation should submit its conclusions to the next (September) Assembly of the League. Unfortunately, the Committee of Experts is not empowered to make any official proposal, as it is merely a Committee of Enquiry instructed by the International Meteorological Committee to submit a report to the committee at its next meeting on September 20.

The International Committee on Intellectual Cooperation will not be able to take a decision before its January session, and any action on the part of the League would be deferred for a whole year.

With a view to obviating such delay—which, in the opinion of the experts, would be highly unde-

sirable—the undersigned have the honor to propose the following line of action:

The International Committee on Intellectual Co-operation might forthwith decide in *principle* to cooperate with the International Meteorological Committee for the creation of an International Bureau of Meteorology in accordance with the suggestions set out above. It might authorize the present sub-committee to act on its behalf as soon as the International Meteorological Committee has formally approved the scheme drawn up by the experts, so that the recommendations in question may be submitted to the Council of the League at its December session.

As regards the question of premises, a decision might be taken by the Committee of Directors of the Institute.

The representative of the International Commission for Air Navigation has promised to see that, at the next (October) meeting of the committee, that organization takes action on the same lines to secure the creation of the International Bureau of Meteorology.

(*Signed*) M. CURIE
H. A. LORENTZ
A. EINSTEIN

## A LABORATORY FOR THE STUDY OF ROCKY MOUNTAIN SPOTTED FEVER

THE state of Montana, in a bill approved by Governor Erickson on March 16, has appropriated $60,000 for the erection of a laboratory at Hamilton for the State Board of Entomology, to be used for the further study of Rocky Mountain spotted fever. The state has also appropriated approximately $25,000 per year for the next biennium to provide for carrying on the work on this problem. Ten thousand dollars per year of this sum is for the study, breeding and colonizing of the French tick parasite (*Ixodiphagus caucurtei* DuBuysson) recently introduced into America by Dr. S. B. Wolbach, of the Harvard Medical School, from the laboratories of the eminent parasitologist, Dr. Brumpt. Dr. F. Larrousse, who is associated with Dr. Brumpt, brought a stock of the parasites to Massachusetts last summer for the purpose of colonizing them on the American dog tick (*Dermacentor variabilis* Say) in Massachusetts.

In addition to the usual features of such laboratories, the building will provide facilities for the rearing of large numbers of infected and non-infected ticks and for the care of large numbers of caged wild rodents. The building will also be provided with rooms and chambers with automatically controlled temperatures and humidity.

The work on spotted fever in Montana is being conducted under an informal plan of cooperation between the United States Public Health Service and the Montana State Board of Entomology. The Public Health Service on U. S. government funds is occupied chiefly at present with a further study of the Spencer-Parker vaccine. The vaccine, made from the bodies of infected ticks (*Dermacentor andersoni* Stiles), was originated two years ago at the laboratory at Hamilton, Montana, and at the Hygienic Laboratory in Washington, D. C., by the two workers from the Public Health Service, and was used last year on about one thousand persons in Montana and Idaho with encouraging results. The Public Health Service is also giving attention to the following: the organism of spotted fever (*Dermacentroxenus rickettsi* Wolbach) and other organisms found in ticks; characteristics of the virus of spotted fever, with particular reference to differences in ticks and in various laboratory and wild animals; relationships of the rabbit tick (*Haemaphysalis leporis-palustris* Packard) in the spotted fever complex; tularaemia, and tick paralysis in man and in domestic animals. The state of Montana, in cooperation with the United States Bureau of Entomology, had previously worked out the life-history and host relationships of the woodtick vector, and the State Board of Entomology is at present concerned mainly with the French tick parasite and with the destruction of ticks by killing the rodent hosts and the dipping of domestic animals. The biology and thermal constants of the parasite are being studied by the entomologist of the board and experiments in colonizing the parasite on ticks in nature are being started this season. The thermal constants of the tick are also being determined and will be correlated with those of the parasite.

The United States Public Health Service is represented at the Montana laboratory by Dr. R. R. Spencer, officer in charge, and entomologist R. R. Parker, special expert. The State Board of Entomology is made up of Dr. W. F. Cogswell, secretary of the State Board of Health, *chairman;* Dr. W. J. Butler, state veterinary surgeon, *member,* and R. A. Cooley, state entomologist, *secretary.* The work of tick destruction is being conducted by Mr. F. J. O'Donnell.

R. A. COOLEY

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## ANNUAL MEETING OF THE AMERICAN MEDICAL ASSOCIATION

THE seventy-eighth annual session of the American Medical Association will be held in Washington, D. C., from May 16 to 20. Eight thousand physicians are expected to attend and be officially welcomed by President Coolidge at the first general evening session, Tuesday, May 17.

The house of delegates will convene at 10 A. M., on Monday, May 16. Meetings of the fifteen scientific sections of the association will be held May 18 to 20. Experts in various branches of medicine will also hold diagnostic clinics for the delegates. The president-elect of the association to be installed in office is Dr. Jabez W. Jackson, of Kansas City, who takes the place of Dr. Wendell C. Phillips.

Memorial services in honor of famous men of the medical profession are to be held on the afternoon of May 16, when wreaths will be placed on statues of Benjamin Rush, Samuel Gross and William Crawford Long; on busts of W. C. Gorgas and Walter Reed, and on a painting of John S. Billings.

Following is a list of the meeting places of the various sections of the association:

*House of Delegates:* Auditorium of the Medical Society of the District of Columbia, 1718 M Street, N. W.

*General Meeting:* Arcadia Auditorium, Fourteenth Street and Park Road.

*Practice of Medicine:* Ballroom, Mayflower Hotel, Connecticut Avenue and De Sales Street, N. W.

*Surgery, General and Abdominal:* Memorial Continental Hall, D. A. R., 300 Seventeenth Street.

*Obstetrics, Gynecology and Abdominal Surgery:* Memorial Continental Hall, D. A. R., 300 Seventeenth Street.

*Ophthalmology:* Hall of Nations, Washington Hotel, Fifteenth Street and Pennsylvania Avenue, N. W.

*Laryngology, Otology and Rhinology:* Hall of Nations, Washington Hotel, Fifteenth Street and Pennsylvania Avenue, N. W.

*Diseases of Children:* Ballroom, Mayflower Hotel, Connecticut Avenue and De Sales Street, N. W.

*Pharmacology and Therapeutics:* Hall, Willard Hotel, Pennsylvania Avenue and Fourteenth Street, N. W.

*Pathology and Physiology:* Hall, Willard Hotel, Pennsylvania Avenue and Fourteenth Street, N. W.

*Nervous and Mental Diseases:* Hall, Hotel Raleigh, Twelfth Street and Pennsylvania Avenue, N. W.

*Dermatology and Syphilology:* Rose Room, Washington Hotel, Fifteenth Street and Pennsylvania Avenue, N. W.

*Preventive and Industrial Medicine and Public Health:* Upper Hall, Concordia Church, 1920 G Street, N. W.

*Urology:* Rose Room, Washington Hotel, Fifteenth Street and Pennsylvania Avenue, N. W.

*Orthopedic Surgery:* Hall, Hotel Raleigh, Twelfth Street and Pennsylvania Avenue, N. W.

*Gastro-Enterology and Proctology:* Upper Hall, Concordia Church, 1920 G Street, N. W.

*Radiology:* Lower Hall, Concordia Church, 1920 G Street, N. W.

*General Headquarters, Scientific Exhibit, Registration Bureau, Technical Exhibit, Motion Picture Theater, Information Bureau and Branch Postoffice:* Washington Auditorium, New York Avenue, Twentieth and E streets.

The scientific exhibit will be on the main floor and stage of the Washington Auditorium; in the same building will also be housed the Technical Exhibits; the Motion Picture Theater will also be located in the spacious gallery of the auditorium. In case of all exhibits, personal demonstrations will be given.

## SCIENTIFIC NOTES AND NEWS

DR. CARL H. EIGENMANN, professor of zoology and dean of the graduate school of the University of Indiana, died on April 24, aged sixty-four years.

DR. F. C. BROWN, assistant director of the U. S. Bureau of Standards, has accepted the directorship of the recently established Museums of the Peaceful Arts, New York City.

PROFESSOR G. D. BIRKHOFF, of Harvard University, has been elected an honorary member of the Edinburgh Mathematical Society.

DR. RUDOLPH MATAS, professor of surgery in Tulane University, has been elected an honorary fellow of the Royal College of Surgeons of England.

PROFESSOR FREDERICK P. GAY, of Columbia University, has been elected an associate member of the Belgian Society of Biology.

MME. CURIE, co-discoverer of radium, has been elected a member of the Academy of Cracow, Poland. Mme. Curie is the first woman to be elected a member, the rules having been especially modified to make this possible.

SIR JAMES BERRY, president of the Royal Society of Medicine, and Sir Frederick Keeble, formerly Sherardian professor of botany, University of Oxford, and distinguished by his work in pure and applied botany, have been elected members of the Athenæum Club for "distinguished eminence in science."

PROFESSOR JULES BORDET, director of the Pasteur Institute at Brussels, has been nominated a corresponding member of the Royal Academy of Spain.

A PORTRAIT of Professor Hobart Amory Hare, painted by Wm. Thomson, was presented to the Jefferson Medical College on May 26, by the class of 1927. Portraits of Professors Thomas McCrae and Albert P. Brubaker, by the same artist, were presented to the college in 1925 and in 1926 by the respective graduating classes.

DR. WILLIAM H. WELCH, professor of medical history at the Johns Hopkins University, presided at a luncheon, given in Baltimore on April 19, by the League of Nations Non-partisan Association.

DR. JOSEPH S. AMES, professor of physics in the Johns Hopkins University, has been named chairman

of the National Advisory Committee for Aeronautics. Dr. Ames was appointed one of twelve original members of the committee by President Wilson in 1915 and has since served continuously.

Dr. Anson Marston, dean of the college of engineering at the Iowa State College, has been named one of three experts who will conduct a survey of drainage work in the Everglades, Florida, and make recommendations for future operations there.

Dr. George H. Monks has resigned as president of the Boston Medical Library, and John W. Barton has been elected to that position. Dr. Monks has been president since 1919.

Dr. C. Montague Cooke, Jr., has been appointed chairman of the directing committee which has recently been organized in Honolulu to make an entomological survey of the countries bordering on the Pacific.

The following officers were elected by the American Society for Experimental Pathology at the recent Rochester meeting: *President,* Dr. David Marine, Montefiore Hospital, New York City; *vice-president,* Dr. E. B. Krumbhaar, University of Pennsylvania Medical School; *secretary,* Carl V. Weller, Ann Arbor, Michigan.

Dr. Alvalyn E. Woodward, associate professor of biology at the University of Maine, has been appointed to a position in cancer research at the University of Michigan.

Dr. Harry A. Cheplin, associate professor of bacteriology in Syracuse University, has joined the staff of the Mulford Biological Laboratories.

Hugh C. Sampson, formerly director of agriculture, Madras, has been appointed economic botanist at the Royal Botanic Gardens, Kew. This appointment has been made possible by a grant of £4,000 for five years from the Empire Marketing Board, through the Ministry of Agriculture and Fisheries.

Professor R. G. Hoskins, professor of physiology at the Ohio State University, has been granted leave of absence and has been appointed research associate in physiology at the Harvard Medical School from April 1 to September 1, 1927.

Dr. A. M. Banta, of the Station for Experimental Evolution, Carnegie Institution of Washington, is spending the spring quarter in the department of animal biology of the University of Minnesota, where he is delivering a course of lectures on genetics and eugenics.

Dr. Donald L. Augustine, assistant professor of helminthology in the department of comparative pathology in the Harvard Medical School, sailed on April 13 for Europe, where he will spend several weeks visiting centers of tropical medicine. Dr. Augustine will arrive early in June in Cairo, Egypt, to make a three months' investigation on certain essential problems concerning ancylostomiasis and other helminthic conditions to be carried out under the auspices of the International Health Board, Rockefeller Foundation, in the laboratories of the Cairo Medical School. Mr. Edward G. McGavran, a third-year student in the Harvard Medical School, will assist in this investigation.

Dr. M. H. Jacobs, professor of physiology, University of Pennsylvania, will deliver the seventh Harvey Society lecture at the New York Academy of Medicine on Saturday evening, May 7. His subject will be "The Exchange of Materials between the Erythrocyte and its Surroundings."

Dr. Russell L. Cecil, New York, will give the twelfth Mellon lecture, under the auspices of the Society for Biological Research of the University of Pittsburgh School of Medicine, on May 12, on "The Specific Therapy of Pneumococcus Pneumonia."

A special meeting of the King County Medical Society, Washington, was held on March 31; the guest of honor was Dr. J. J. R. Macleod, of the University of Toronto Faculty of Medicine, whose subject was "Some Recent Researches on the Ductless Glands."

Dr. Edwin B. Frost, director of the Yerkes Observatory, delivered an address entitled "Beyond the Milky Way," at Washington and Lee University, on the evening of April 4.

Dr. Francis G. Benedict, director of the Nutrition Laboratory of the Carnegie Institution of Washington, Boston, lectured on "Recent Studies in Human and Animal Metabolism" at Wellesley College on the evening of April 14, and at Teachers College, Columbia University, on the afternoon of April 22.

Dr. Tadasu Saiki, director of the Imperial Government Institute for Nutrition, Tokyo, Japan, lectured on April 22 and 23 at Stanford University Medical School on "Modern Currents in the Studies of Nutrition" and "Practical Application of Studies in Nutrition." The lectures are given under the Morris Herzstein Lectureship on diseases of the Pacific Basin, including tropical diseases.

Dr. Frederick W. Hodge, ethnologist of the Museum of the American Indian, New York City, was the principal speaker at a joint meeting of the Washington Academy of Sciences and the Anthropological Society of Washington, on April 21, when he gave an address on "The Zuni Indians of New Mexico."

Among the six busts to be unveiled in the hall of fame at New York University on May 5, at 3:15

o'clock, is one of John James Audubon, the ornithologist. The bust, executed by A. Sterling Calder, was presented by the American Geographic Society, the Museum of the American Indian, The Hispanic Society of America, The Numismatic Society and the American Academy of Arts and Letters. The principal speaker will be Dr. Frank M. Chapman, of the American Museum of Natural History.

Princeton University has established a professorship in memory of Charles A. Young, who was professor of astronomy at the university from 1887 to 1908. Professor Henry Norris Russell, director of the Princeton observatory, has been made the first incumbent.

The United States Civil Service Commission announces a competitive examination for junior chemical engineer, applications for which must be on file not later than May 14. The examination is to fill vacancies in various branches of the service throughout the United States at an entrance salary of $1,830 a year.

At the recent meeting of the American Chemical Society in Richmond announcement was made of the appointment of Professor Neil E. Gordon, of the University of Maryland, and Professor B. S. Hopkins, of the University of Illinois, as members of the board of directors of the Institute of Chemistry, the first session of which is to be held at the Pennsylvania State College this summer. Professor S. W. Parr, of the University of Illinois, has been chosen a member of the executive committee of the society. New sections of the society have been authorized in Porto Rico and in Ada, Ohio.

The spring meeting of the Indiana Academy of Science will be held at New Harmony, Indiana, on Thursday, Friday and Saturday, May 12, 13 and 14. This meeting celebrates the beginning of scientific work in Indiana. Say, Maclure, Troost and several other scientists came to New Harmony in 1826 and during the years immediately following laid the foundation for the research which was carried on here during the following half-century. The program for this meeting will include a discussion on the history of New Harmony by an authority on Indiana history and a talk on local points of interest and traditions by a citizen of New Harmony. These talks will be followed by a tour of the city and vicinity.

The Second British Empire Mining Conference takes place in Canada this year. The meetings will begin at Montreal, on August 22. Some 500 members from overseas and from various units of the Empire have already agreed to attend, besides local members from all the provinces in British America. Sectional gatherings, excursions and meetings will be held in Halifax, Quebec, Montreal, Ottawa, Toronto, Winnipeg, Edmonton and Victoria (Vancouver Island). The first conference took place in 1923 at Wembley Park, and proved a great success. Sir Robert Horn is president of the conference for 1927.

The fourth International Congress of Theoretical and Applied Limnology is to be held at Rome in September, 1927. The exact dates and details of the program are to be published later. It is proposed to organize limnological excursions around Rome and the regions of central and northern Italy and Naples, while a limnological exhibition will be held in Rome and a fishery exhibition in Como.

The John and Mary R. Markle Foundation has been incorporated with an initial fund of $3,000,000, given by John Markle, retired coal operator. The purposes of the foundation include: creation and maintenance of medical research centers, hospitals, charitable institutions and educational agencies, study of disease and development of methods of eradication, support to libraries and assistance to destitute individuals.

Dr. Walter B. James, who was, at the time of his death on April 6, president of the Long Island Biological Association, which maintains the Biological Laboratory at Cold Spring Harbor, has bequeathed $5,000 to the association, the income to be used for its purposes.

A dental clinic, identical in character with that at Rochester, N. Y., will be constructed in London as a result of a gift of £300,000 by George Eastman, of Rochester, head of the Eastman Kodak Company. The clinic will be associated with the Royal Free Hospital in Gray's Inn Road in a building adjoining the hospital.

Yale University, New Haven, announces that it is prepared to found a biochemical institute, where physicians, chemists, biologists and physicists may combine their effort in attacking biochemical problems. The necessary funds have not, however, been secured. The Sterling laboratory of chemistry at Yale is now taking the first step to manufacture a new sulphur constituent of the blood, thus far known as "constituent X," in sufficient quantities for thorough biologic and clinical experiments.

An anonymous donor has put the sum of 100,000 francs at the disposal of the Société de Neurologie of Paris to be awarded to the discoverer in the course of the next two years of a cure for disseminated sclerosis. Candidates must be members of the Société de Neurologie or at least of French nationality.

AN anonymous donor has given the directors of the Chicago Municipal Tuberculosis Sanitarium $1,000 to be awarded to some physician, scientist or laboratory worker of Chicago who develops during the year the most useful discovery for the prevention or cure of tuberculosis.

THE Zoological Society of San Diego has completed a zoological hospital and research institute. This building is especially designed and fully equipped for all types of biological research. The purposes of the building are as follows: (a) To serve as a hospital for the animals in the local zoological garden. (b) To serve as a place where animal diseases can be systematically studied. (c) To afford to students of biology a laboratory, where they can pursue their studies with unusual opportunities. The building provides accommodation for thirty research workers. Laboratory and equipment will be supplied these men without charge, and they will be at liberty to follow any type of work in which they are interested. This building was sponsored by a group of progressive physicians. It is ideally located, and the only one of its kind in this part of the country. It is hoped that in time it will develop into an important center of scientific investigation.

A NEW Field Museum expedition to the Alaska Peninsula will leave Seattle on May 1 to collect a group of Kodiak bears and other specimens of the territory. Alexander H. Revell, Chicago merchant, will finance the enterprise, to be known as the Alexander H. Revell Field Museum expedition to Alaska of 1927. Robert W. Tansill, another Chicago business man and big-game hunter, will be in charge of the party.

SIGMA XI FELLOWSHIPS for 1927–28 will be awarded early in June. These fellowships are available for workers in all fields of science. There are no restrictions as to the university or the country in which the holder is permitted to work. Applications should be made before May 20 to Dean Edward Ellery, Union College, Schenectady, N. Y.

THE College of Mines of the University of Washington offers five fellowships for research in coal and non-metallics in cooperative work with the United States Bureau of Mines. The fellowships are open to graduates of universities and technical colleges who are properly qualified to undertake research investigation. The value of each fellowship is $720 to the holder, for twelve months beginning July 1. Fellowship holders pay tuition and laboratory fees, but are reimbursed for the amounts so expended; they register as graduate students and become candidates for the degree of master of science in the proper subject, unless an equivalent degree has previously been earned. The purpose of these fellowships is to undertake the solution of various problems being studied by the United States Bureau of Mines that are of especial importance to the State of Washington, the Pacific Northwest and Alaska. The investigations consist principally of laboratory work directed largely by the bureau's technologists.

UNDERGRADUATE students of the Sheffield Scientific School of Yale University have founded the *Yale Scientific Magazine,* which will be devoted to the scientific and engineering research work and news of the school. The publication will appear quarterly, its organization plan having been approved by the Yale Corporation and by the faculty of the Sheffield Scientific School. Its editors will be undergraduates, who will be assisted by a faculty adviser and supervised by an advisory board composed of men of national reputation in various fields of science.

ACCORDING to the *Journal* of the American Medical Association the Rockefeller Foundation has granted the request of the public health service of Haiti to award scholarships to a number of young physicians to enable them to do postgraduate work in some large medical centers of the United States and Canada. About eight fellows had been appointed up to March 24. The object is to improve the teaching facilities of the National Medical School.

AN exhibition has been opened to the public at the Science Museum, South Kensington, London, which affords some indication of the assistance given to British metal industries by the British Non-Ferrous Metals Research Association, which, with the cooperation of the industry through membership of the association, carries on research work and maintains an information service. According to the *Electrical World* the exhibition is concerned chiefly with eight of the association's researches, chosen from among eighteen major investigations at present in progress. Those selected are typical of the character and wide range of the work which is being done, and the exhibits have been set out to give some idea of the scope of each piece of work and the progress which has so far been made. They include the effect of impurities on the properties of copper, gases in copper castings, the jointing of metals, die casting alloys and "wiped" plumbers' joints.

FORMAL steps are being taken to establish, within the Stanford University campus territory, one of the finest botanical gardens in the world. The board of trustees has given its consent to the appropriation of 1,000 acres of ground for development whenever sufficient funds have been obtained to assure the success of the project. The gardens will be a preserve for plant and animal life, a place where experiments

in plant development which require generations of effort can be carried on, an exhibition garden, a training school for gardeners, a public park and an experimental laboratory for both professors and students. One million dollars will be necessary before a start can be made, according to Comptroller A. E. Roth. The ultimate development of the garden would require an endowment of from $6,000,000 to $10,000,000. The project is being advanced at present by a group of scientists and others under the organization name of Pacific Botanical Gardens, whose executive committee includes Comptroller A. E. Roth and Professor G. J. Peirce, of Stanford; Professor E. B. Babcock and Comptroller Robert Sproul, of the University of California; Dr. H. M. Hall, of the Carnegie Corporation; George C. Roeding, of Fresno, and Milton B. Drury, secretary of the Save-the-Redwoods League.

Announcement is made of a gift of $200,000 by Mr. J. P. Morgan to the Neurological Institute, New York, for the establishment of a fund for research and treatment of encephalitis. The fund will be administered through the new hospital of the institute to be erected as part of the Columbia-Presbyterian medical center in New York City. A complete floor will be equipped, including a ward of forty-eight beds, for the treatment of the disease.

## UNIVERSITY AND EDUCATIONAL NOTES

The General Education Board has appropriated $750,000 towards the sum of $1,500,000 required by Yale University for the construction of a new surgical laboratory and the extension of the pathological laboratory.

Mr. and Mrs. John Roberts have given to the University of Chicago $1,000,000 for the construction and endowment of a hospital for children.

Harvard University will get the bulk of the estate of Dr. Charles A. Brackett, long oral pathologist in its dental schools, amounting to nearly half a million dollars.

President Edward M. Lewis, of the Massachusetts Agricultural College, has been elected president of the University of New Hampshire, to succeed Dr. Rudolph D. Hetzel, who recently resigned to become president of the Pennsylvania State College.

Roger Lowell Putnam was named by the late Guy Lowell his successor as trustee of the Lowell Observatory, in accordance with the provisions of the will of Percival Lowell, who founded the observatory at Flagstaff, in 1894, and there, until his death in 1916, pursued the study of astronomy—with particular regard to the planets. Mr. Putnam is a nephew of the noted astronomer.

The department of pathology of the University of Pennsylvania has been reorganized under the chairmanship of Dr. E. L. Opie, who is, however, to continue his work at the Phipps Institute. Dr. E. B. Krumbhaar has resigned his posittion as director of laboratories at the Philadelphia General Hospital to follow Dr. Allen J. Smith as professor of pathology, and Dr. Herbert Fox has been appointed professor of comparative pathology. Dr. Baldwin Lucké has been promoted to an associate professorship.

Dr. Elmer Funkhouser, instructor in pathology at the medical school of Indiana University, has been promoted to be an associate.

At the University of Bristol Dr. William Edward Garner, of University College, London, has been appointed professor of physical chemistry in succession to Professor J. W. McBain. In the department of physics Dr. J. E. Lennard Jones, reader in mathematical physics, has been promoted to the professorship of theoretical physics.

Professor Hans Wintersten has succeeded Professor Karle Hürthle in the chair of physiology at Rostock.

## DISCUSSION AND CORRESPONDENCE

### PRESSURE DECOMPOSITION AS A SOURCE OF SOLAR ENERGY

In a recent paper[1] Bridgman points out that if atoms are subjected to extremely high pressures, the superstructure of quantum orbits may give way, freeing the kinetic energy of the orbital electrons which would then become available as heat. Bridgman asks, "Has this been considered as a source of stellar energy?" I have made the following computations.

The kinetic energy of an electron in its orbit, including the relativity correction, which becomes appreciable for K-electrons in elements of high atomic numbers, is given by

$$E_{kin} = c^2 m_0 \left[ \frac{1}{\sqrt{1 - \frac{\alpha^2}{n^2} Z^2}} - 1 \right] \qquad (1)$$

where c is the velocity of light, $m_0$ the statical mass of an electron, Z the atomic number and n the azimuthal quantum number. $\alpha$ is the so-called constant of fine structure, equal to $\frac{2\pi e^2}{ch}$, e being the charge on the electron and h Planck's constant. Numerically,

$$\alpha = 7.29 \times 10^{-3} \text{ and } \alpha^2 = 5.31 \times 10^{-5}$$

[1] *Phys. Rev.* 29, 188, 1927.

For all elements where $\frac{\alpha^2}{n^2}Z^2$ is small, formula (1) reduces to

$$E_{kin} = \frac{c^2 m_0}{2} \frac{\alpha^2}{n^2} Z^2 \tag{2}$$

The electron revolving in the first Bohr circle of the hydrogen atom will have a kinetic energy of

$$E_{kin} = 2.1 \times 10^{-11} \text{ ergs} \tag{3}$$

Since there are $6 \times 10^{23}$ hydrogen atoms per gram of the substance, the total energy which would be released by pressure decomposition is

$$E_{kin} = 1.26 \times 10^{13} \text{ ergs/gm} = 3.0 \times 10^5 \text{ cal/gm} \tag{4}$$

Since the sun radiates approximately 1.5 calories per gram per year, the new source of energy, for a sun composed entirely of hydrogen, could be relied upon to keep the sun going for a period of only $2 \times 10^5$ years.

This figure will be increased when atoms of higher atomic weight are considered. To evaluate the other extreme, consider uranium, with its ninety-two orbital electrons. Summing the kinetic energies for the respective rings, taking Z, in each case, equal to the effective atomic number, we obtain an approximation to the total energy of all the electrons in the atom. The computed value is $1.4 \times 10^{-6}$ ergs per atom, or

$$E_{kin} = 3.5 \times 10^{15} \text{ ergs/gm} = 8.5 \times 10^7 \text{ cal/gm} \tag{5}$$

The foregoing computations show that the contribution of complete pressure decomposition to the life of the sun is less than $6 \times 10^7$ years. The accepted age of the earth is of the order of $10^9$ years, hence this theory, as well as any other which fails to furnish energy for at least that length of time, must be discarded as inadequate though, of course, a small fraction of the solar radiation may be attributed to that source.

DONALD H. MENZEL

LICK OBSERVATORY,
JANUARY 26, 1927

## "COMMENSALISM" OF A SEA ANEMONE AND A SEA URCHIN[1]

DURING the summer of 1926, while collecting along the south shore of Cienfuegos Bay, Cuba, I encountered an interesting example of commensalism which has hitherto escaped notice. This was the presence of the sea anemone, *Aiptasia tagetes* D. and M. on the aboral surface of the test of the sea urchin *Diadema*. The pedal disc of the sea anemone was about 8 mm from the anal opening of the sea urchin. When observed in the living state the tentacles nearest the anus were being moved over the anal opening and presumably any excreta could thereby easily be transferred to the mouth of the actinian.

[1] Contribution No. 4 from the Harvard Biological Station, Atkins Foundation, Soledad, Cienfuegos, Cuba.

While both forms are very common here—the sea anemone encrusting rocks at or just above low water mark, and the sea urchin plentiful in shallow water—only two pairs were found in this relationship and these within eight meters of each other. The sea anemones were of the same size, 28 mm in height and 15 mm in diameter. The sea urchins were not full grown—the test of one measured 52 mm in diameter, while the second measured 44 mm. The distance of the actinian from the anus was the same in each case.

The advantage which the actinian derives from this association is clear—a constant food supply during the life of the sea urchin. The sea urchin, on the other hand, may be regarded as a passive host deriving no advantage and suffering no disadvantage. The initial contact of the planula with the sea urchin and its attachment was undoubtedly fortuitous.

BENJAMIN KROPP

ZOOLOGICAL LABORATORY,
HARVARD UNIVERSITY

## PRESERVATION OF NATURAL AREAS

IN addition to the preservation of suitable areas of virgin forest from the standpoint of saving the trees themselves in their natural growing conditions, as pointed out by Dr. Van Name,[1] the preservation of examples of virgin forest soil, including litter and humus, would seem to be an equally important object. The soil is the foundation of forest growth just as it is of other vegetation; if trees of the type found in virgin forests can not be replaced in some cases for several centuries, what of virgin forest soils? As Dr. Van Name mentions, removal of dead trees for firewood and picnicking of tourists do not meet the requirements for preserving natural conditions. All litter and dead timber should be left untouched, and no disturbance (such as pushing over old stumps) should be permitted; nothing should be taken away, and nothing added.

In forestry, as in other fields, we have been wont to turn to Europe for examples, good or bad. The unfortunate result of the lack of preservation of virgin areas in central Europe was illustrated recently when Professor Hesselman, of the Swedish Forest Experiment Station, wished to study humus-types in virgin forests for comparison in connection with his extensive investigation of humus in coniferous forests.[2] Aside from some inaccessible parts of

[1] SCIENCE, n.s. 65: 173, No. 1677, 1927.

[2] Hesselman, Henrik. "Studier över barrskogens humustäcke, dess egenskaper och beroende av skogsvården." (Studies of the humus cover in coniferous forest, its characteristics, and dependence on forest conservation). Meddelanden från Statens Skogsförsöksanstalt 22: 169–552, 1925.

Lithuania and the Carpathians, the only natural area he could locate was the Böhmerwald on Mt. Kubani near the Bavarian boundary. This had been preserved as a natural reservation by Count Adolph von Schwarzenberg and his family, and comprised an area of forty-six hectares (113.6 acres) embracing one major forest type.

The litter, humus and soil conditions in virgin forests may not be ideal—are often very poor from the view of commercial production, and could be bettered by cultural treatment—but certainly our intensive use of forest land has not reached such a point that we can not afford to reserve some areas of reasonable size in each vegetational, climatic or soil type, in a state of natural productiveness or unproductiveness. Our greatest luxuries are the millions of waste and devastated acres. As far as the national forests are concerned, it would not seem to require any great modification of policy, and surely but little financial deprivation to set aside areas in each type as permanent "control" areas in the government forest research program. It would not be necessary to lay out any exact plots, or carry out any elaborate studies at once. The areas should not be too accessible; but they should be catalogued and available for properly qualified persons to study under certain restrictions. No one can tell when such areas will be called upon to furnish solutions for the most practical kinds of problems. The types have been studied and classified with few exceptions, their locations are known, and men are available, well-fitted to carry out the project. It is obviously a function to be performed by some public agency, and the sooner it is done the better.

HENRY I. BALDWIN

BERLIN, NEW HAMPSHIRE

## WHEN IS MID-WINTER?

ALTHOUGH December 22 or thereabouts is the shortest day of the year, it never seemed to me that it was justly entitled to the name of "mid-winter." Recently I made a careful study of Weather Bureau records of St. Paul and Minneapolis to find out when, in the sense in which the term ought perhaps to be used, the real middle of cold weather occurs in this climate. It was found that the lowest average daily temperatures occur in January; in fact, the records showed that from the 10th to the 23rd of January, inclusive, the average temperature was 11° F. and that before and after these dates the daily temperatures were higher. As this did not give a satisfactory answer, the problem was approached in another way. It was noted that September 20 and May 18 both had average temperatures of 59 degrees. Between these two dates, it was assumed that, if the average daily temperatures were each subtracted from 60 and the differences totaled, the date before and after which the half totals equaled each other could properly be called the middle of winter. By this method of calculation, January 18 was found to be the date sought. Sixty degrees was chosen on the assumption that at about that temperature in the average year fires would be built and in the spring when the temperature had reached 60 degrees, fires would be allowed to die. Is it not fair to suppose that on January 18, in the climate of the Twin Cities, the coal pile should be half gone, if we may assume that on the average wind velocities and other conditions are no more effective before that date than after that date in eating up fuel? This date is not in accord with the old proverb, "Half your meat and half your hay should be in store on Candlemas Day" (February 2).

About midnight, July 19, exactly one half year has elapsed, and it is perhaps proper to apply the term "middle of summer" to this date, although from the 17th to the 25th the average temperature remains at its highest point of 73 degrees in the Twin Cities, which would bring mid-summer two days later by this process of reasoning.

It would be interesting to know when mid-winter, as above defined, occurs in such climates as Buffalo, New York, Florida, Los Angeles, Seattle and Southern England.

CHARLES H. BRIGGS

MINNEAPOLIS, MINN.

## THE NEWTON BICENTENARY

THE notable scientific distinction of 1927 being the fact that it is the bicentenary year of Newton's death, it would seem desirable to mark it, where possible, by tributes to his memory more real than merely literary and festival celebrations. The preparation of a revised complete edition of his writings is, it seems, not to be looked for, however appropriate and overdue such an undertaking may be; but a return to their direct study, and to the recital and employment of methods and results contained in them, that at least is practicable, and would be a generous homage on their part, if writers and teachers would adopt it.

One of his methods, with which a start might well be made, is Newton's theory of resistance—that theory which Cotes characterized as "his noble theory of resistances." Noble it undoubtedly is, yet none the less it has for long years been consistently passed over, if not forgotten, for too few of us read the "Principia" now, though we one and all allow it to be a scientific classic that can never die.

In place of Newton's "noble theory" we have sub-

stituted a so-called fluid-friction theory—imperfectly defined, little understood and based on fallacious reasoning.

To compare for a moment the two theories: our fluid-friction theory lays it down fundamentally that a fluid devoid of viscosity offers no resistance to uniform motion through it. This theory has now enjoyed a long scientific inning, but it has not yet enabled any one (to my knowledge) to calculate the resistances of bodies of the simplest form.

Newton, in his theory, lays it down fundamentally that a fluid devoid of viscosity *does* offer resistance, and that such resistance varies as the density of the fluid and as the square of the velocity of the body moving uniformly through it.

In the "Opticks" (Query 28)—in 1718 and therefore his matured view—he is more precise, and says the resistance of a sphere "which arises from the attrition of the parts of the medium is very nearly as the diameter, or at most, as the *factum* of the diameter and velocity of the spherical body together. (Thus anticipating what we call "Stokes Law.") And that part of the resistance which arises from the *vis inertiae* of the matter is as the square of that *factum*. And by this difference the two sorts of resistance may be distinguished from one another in any medium; and these being distinguished it will be found that almost all the resistance of bodies of a competent magnitude moving in air, water, quicksilver, and such like fluids with a competent velocity, arises from the *vis inertiae* of the "parts of the fluid." And a little later he again says:—"The resistance of water arises principally and almost entirely from the *vis inertiae* of its matter."

The calculation of any resistances by our fluid-friction theory being admittedly impossible, would it not be an appropriate tribute to Newton's memory to give his own *peregregia theoria* a place in his bicentenary year? Resistances *can* be calculated by Newton's theory to a high degree of accuracy, as was shown by Colonel Duchemin more than eighty years ago.

For another comparison, take the case of water flowing out through a one-inch circular thin-walled orifice. Calculating in the ordinary manner we obtain a figure which we may call 100, while we know that by experiment we shall get a figure about 62.5. We have accordingly to employ a coefficient of 0.625 before our result is of any value at all. Such a method scarcely deserves to be called scientific. The condition is really worse than this, for our coefficient is not even constant but must be varied as the diameter varies.

On the other hand, by Newton's theory the discharge for a one-inch orifice can be calculated directly to within the second place of decimals of the experimental amount. Surely we should be consulting our own practical advantage, as well as honoring Newton's memory, by at least finding a place for his theory—a theory which leads at once to a correct result—alongside one which is useless without what may be called a "judging" coefficient.

If it be asked why it is assumed that a fluid devoid of viscosity offers no resistance to uniform motion, the answer may be found in the first three lines of Lamb's "Hydrodynamics."

R. DE VILLAMIL

WEST HAMPSTEAD,
ENGLAND

## SCIENTIFIC BOOKS

*American Silurian Crinoids.* By FRANK SPRINGER. Publication No. 2871, Smithsonian Institution, 236 pages, 32 plates, 1926.

MANY-SIDED Frank Springer, born in 1848 in Iowa, educated there and admitted to the bar in 1869, has long been America's foremost authority and the world's most prolific worker in the field of fossil crinoids. Beginning his scientific career in 1877 with an adopted son of Iowa, Charles Wachsmuth, their joint publications continued until 1897 (sixteen titles). Since then, Springer has carried on his studies of crinoids alone, and has added to his bibliography fifty-seven scientific titles, besides forty-six other miscellaneous papers having to do with law and public affairs. His results are fundamental in crinoid morphology and taxonomy. Since 1873 he has been a citizen of New Mexico, where he became one of the state's leading men and also amassed a fortune. A good part of the latter was used in getting together the largest collection of crinoids, blastoids and cystids anywhere, and after describing and illustrating these rarities as no other worker has, he gave them unencumbered to the nation through the Smithsonian Institution. Crinoids, and especially whole ones, are usually very rare fossils, but when good leads are gone after with pick, shovel and powder, as many have been under Springer's direction, the results are astonishing, as is again attested in the beautiful and monumental work under review. The drawings, by Kenneth M. Chapman, are very fine, and the half-tone reproductions are the best we have ever seen.

In "American Silurian Crinoids," 198 forms (seventy-four new) in sixty-three genera (seven Camerata are new) are discussed and illustrated, but not all these are of Silurian time, nor are all the American forms included. Rather is it the species found in Tennessee, Indiana and Kentucky that are dealt with,

so that the total number of forms of this period in our country must now exceed three hundred. A number of species "are scarcely distinguishable from English and Swedish forms," and this is all the more remarkable in these very intricately plated animals. Out of the sixty-three genera, thirty-three are common to Europe and America. The reviewer is further surprised to see no fewer than nine species (seven are of *Pisocrinus*) common to the southern and northern Silurian seas of the United States, showing that these waterways must have been in open connection.

The monograph also describes those most anomalous of all crinoids, the Calceocrinidæ, some of which have been called "dead men's fingers." Here the crown, starting with the normal number of radii (five), gradually eliminates two of them, and the head is reversed or recumbent upon the stem, a peculiar evolution which the author thinks was due to the stalk or stem being prostrate on the ground, with the crown in the horizontal attitude when feeding. Springer brings together all that is known of these highly specialized American and European crinoids (four genera and twenty-eight species), ranging from the middle Ordovician to the Mississippian (Keokuk), and successfully traces and illustrates their trends of evolution. The Calceocrinidæ are, he says, "an example of evolutionary modification with the process clearly visible, that is without precedent among the crinoids" (p. 71). Their singular modification out of normally developed Heterocrinidæ is due to the fact that "the base is united to the radials by a hinged muscular articulation, allowing motion of the crown above the base up and down in the plane of its bilateral symmetry" (p. 88).

Would that the human world had more men like Frank Springer!

CHARLES SCHUCHERT

## SPECIAL ARTICLES

### PROPERTIES OF SUBSTANCES IN THE CONDENSED STATE AT THE ABSOLUTE ZERO OF TEMPERATURE

IN a paper read at the Philadelphia meeting of the American Association for the Advancement of Science the writer deduced from results of an axiomatic character a result which may be stated in the form that the controllable internal energy and entropy of a substance or mixture in the condensed state at the absolute zero of temperature are zero, and that this zero corresponds to the mathematical definition of a minimum in the Calculus.[1] The result includes the third law of thermodynamics and Nernst's heat theorems. A number of deductions may be made from it which are of considerable interest and importance. These can not be deduced, it may be mentioned, from the third law of thermodynamics or from Nernst's heat theorems. The deductions of some of these results are given in a paper read at the February meeting of the Physical Society held in New York City, of which a few will be given here because they open up a new field of experimental research.

It is shown that besides

$$C_v = 0 \qquad (1)$$

for any substance or mixture in the condensed state at the absolute zero of temperature, we also have

$$\left(\frac{\partial' C_v}{\partial' T}\right)\frac{1}{v} = 0 \qquad (2)$$

where $C_v$ denotes the specific heat at the constant volume v, and T the absolute temperature. Experimental evidence of the truth of these two equations already exists. According to Debye's formula for the specific heat of a monatomic solid, which is found to agree well with the facts, the specific heat near the absolute zero of temperature is given by

$$C_v = a_1 T^3 \qquad (3)$$

where $a_1$ is a constant. This equation has been specially investigated by Kammerlingh Onnes and found to agree well with the facts.[2] Equations (1) and (2) evidently agree with it. Experiments on the specific heat of mixtures near the absolute zero of temperature besides on monatomic solids would be very desirable.

If U denotes the internal energy of the substance or mixture, and it may be expressed as a series of integral powers of T by Taylor's theorem,

$$U = a_2 T^3 \qquad (4)$$

near the absolute zero of temperature, according to equations (1) and (2), where $a_2$ is a constant.

It is also shown that besides

$$p = 0 \qquad (5)$$

we have

$$\frac{dp}{dT} = 0 \qquad (6)$$

and

$$\frac{d^2p}{dT^2} = 0 \qquad (7)$$

where p denotes the vapor pressure of the substance or mixture. Therefore, if p may be expressed in integral powers of T by Taylor's theorem

$$p = a_3 T^3 \qquad (8)$$

near the absolute zero of temperature, where $a_3$ is a

[1] See SCIENCE, Feb. 25, 1927.

[2] *Comm. Phys. Lab.*, Leiden, No. 147 (1915).

constant. The writer is not aware of any experimental evidence of the truth of this equation. It affords an opportunity of putting to an important use the methods of measuring low pressures which are being perfected at present.

It is also shown that

$$\frac{dv}{dT} = 0 \tag{9}$$

$$\frac{d^2v}{dT^2} = 0 \tag{10}$$

Therefore, if the volume v of the substance or mixture in the condensed state may be expressed in a series of integral powers of T by Taylor's theorem,

$$v = v_0 + a_4 T^3 \tag{11}$$

near the absolute zero of temperature according to equations (10) and (9), where $v_0$ denotes the volume of the substance or mixture in the condensed state at the absolute zero of temperature, and $a_4$ is a constant. The foregoing equation could be tested experimentally without encountering insuperable difficulties, probably best by an optical method.

It will be of interest to point out that if equations (1) and (2) can be proved experimentally for a number of substances—Kammerlingh Onnes's experiments furnish the proof for some substances—equations (6), (7), (9) and (10) can be shown to hold for these substances by means of well-known thermodynamical formulae and the Calculus.

It may also be mentioned that it is shown that equation (1) holds for a mass of matter in the gaseous state at infinite volume at the absolute zero of temperature. The experiments of Scheele and Heuse[3] on the specific heat of helium at constant volume, who found that it decreased with decrease of temperature, support this deduction.

The foregoing results, since they were deduced by the help of thermodynamical formulae, will apply to a substance or mixture only if it is in a state that it can be passed through a thermodynamical cycle, *i.e.*, if it is in perfect thermodynamical equilibrium.

R. D. KLEEMAN

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## PROFITS DERIVED FROM SEGREGATING COLLEGE STUDENTS ON THE BASIS OF ABILITY

Two experiments, to determine both the qualitative and quantitative advantages that may be derived particularly by superior college students when segregated on the basis of ability, have recently been completed at the University of Michigan under the direction of Professor Henry W. Miller, head of the department of mechanism and engineering drawing, of the college of engineering. An exhaustive report has been prepared on these experiments by Professor Miller for the senate of the university, which began consideration several years ago of the question, "What can we do to further benefit our superior students?" A limited number of copies of this entire report are available for distribution to those interested in the more detailed discussions of the procedure in conducting the experiments, the incidental findings, and in the graphs showing the performance of the various classifications of students in all their college work, and in particular subjects. The complete report will be published soon in *School and Society*. A brief description of the experiments and the more pertinent findings are given herein.

The department of mechanism and drawing at Michigan is an independent department of the college of engineering and gives all the general courses in engineering drawing to some eight hundred freshmen and sophomores yearly. The department was in the process of revising all its courses, methods of teaching, etc., and felt that an effort should be made to determine the nature and extent of the benefits that might be derived by the students if they were taught in homogeneous groups made up on the basis of ability, rather than in the usual heterogeneous groups. Interest centered particularly in learning what benefits the superior students might derive therefrom and the extent.

Descriptive geometry was chosen as the best test medium for these experiments, for reasons which those having a knowledge of this subject can readily understand. This subject is given to the freshman class in its second semester. All students being tested had, therefore, completed one semester's work in college and a double test could be conducted, the one to determine the nature and extent of advantages from segregation on the basis of ability, and the other to learn whether the quality of work done on previous college courses can serve as a satisfactory basis of segregation.

### METHODS OF CONDUCTING THE EXPERIMENTS

At the end of the first semester and before the beginning of the second, the general averages made by the freshman engineers on all their first semester subjects were computed and the names of the men listed in the order of their averages. In computing these averages the mathematical value of the letter grade for each subject was multiplied by the number of credits for that subject and the sum of all the products divided by the sum of the credits. In general the grade "A" is given for a record of from 90 to 100 per cent., "B" for 80 to 90, "C" for 70 to 80, and "D" for 60 to 70. The university gives a value of 4 to the grade "A," 3 to "B," 2 to "C" and 1 to "D." In computing general averages, then, the four credits for any sub-

[3] *Ann. der Physik.*, 40, p. 484 (1913).

ject on which a grade of "B" had been secured were multiplied by the value "3" of this grade. The general averages ranged from 4 down; very few 4's.

The students in descriptive geometry are enrolled in six main groups and each of these groups is later subdivided into sections of from twelve to fifteen. On completion of the enrollment, those students of each main group who had a general average on all first semester work of 3 or more were segregated into one section. Those having a general average of less than 1.5 were segregated into another section and the remaining students were divided into sections of from twelve to fifteen. In both experiments it was found that in a group of from sixty to seventy students there were about twelve with averages of 3 or more, and another twelve with averages under 1.5.

In each experiment the low and high sections were given to six instructors with long experience in the teaching of this subject, so that as many as possible of highly trained teachers might have the opportunity to compare results, reactions, necessary teaching methods, capacities, qualities, etc., of the highest and the lowest of the students.

### Results and Conclusions

(1) Under the above scheme of segregation about twenty per cent. of freshman engineering students were classed as "high" and another twenty per cent. as "low."

(2) The low students (in the two experiments under discussion) are in the main the children and grandchildren of American-born men and women. They also are the sons of the better educated and more prosperous parents.

(3) A surprisingly large percentage of the high students are the grandsons of foreign-born men and women. They are the sons of parents having either but little education or none. In the main they are the sons of parents whose education does not include the high school. Twenty-four per cent. are the sons of parents who had less than a grammar school education.

(4) The low students had made relatively poor records on their preparatory school work, particularly on mathematics, physics and chemistry. They made poor records also on *reduced* loads of work during both semesters in college.

(5) The high students had made an excellent or good record on their preparatory school work, particularly in mathematics, physics and chemistry.

(6) Outside employment to earn a part of the school expenses, room, board or money seems to have no noticeable effect on scholarship.

(7) Over ninety per cent. of the high students returned for a second year. Only forty per cent. of the low students returned.

(8) The instructors of high and low students learned facts about the qualities, capacities, etc., of such students when segregated that they had not been able to learn when the students were mixed.

(9) Students in low sections quickly develop a confidence in what ability they possess that they do not seem to develop in sections containing appreciably keener men. The idea that they are inspired by more capable men seems to have no support. They do better work when by themselves than when in mixed sections.

(10) The instructor quickly adjusts all discussions and the pace in high sections to the capacities of such men. All discussions are in terms of their keen understanding; the entire class time is spent on just them, all of their time is employed profitably. They are worked at a pace to develop their capacities to the maximum.

(11) Low students are lacking in the quality of vision, powers of analysis and coordination; they are superficially bright and ingenious, but their tenaciousness of purpose and capacity for sustained mental effort are so low that their curve of effort has a steadily downward trend.

(12) High students rate notably high in powers of perception, analysis and correlation, tenaciousness of purpose, capacity for sustained mental effort and inclination to succeed.

(13) The amount of energy required of the instructor of low students to overcome mere friction, to make up for deficiencies, lack of inclination to succeed, etc., is so great that no instructor can gauge it until he has taught a group of such men.

(14) There is no friction to be overcome in getting a group of high men under way. The instructor is constantly exhilarated in keeping up to their pace. He serves for them as a guiding rather than as a driving agent.

(15) High students are able to finish exercises designed to fit a mixed class in about three fourths of the average time of a mixed class.

(16) Low students profit when segregated under three factors: *Intelligence, personal contact* with the instructor and *confidence development*. No measure has yet been effected of the extent of gain under any of these factors.

(17) High students profit when segregated under two factors: *Time,* to the extent of about twenty per cent., and *intelligence,* to the extent of at least ten per cent. They profit also under two other factors: *Content of course,* and *personal contact* with the instructor and a group of men of their own kind, but the extent of this profit has not yet been measured. The measured and agreed upon profit is at least thirty per cent.

(18) The method of segregating students on the basis of the quality of previous college work seems satisfactory because all sections so arranged performed, in the test subject, and in the entire group of second semester subjects, exactly as rated.

(19) No harm is done to any grade of students under segregation and after one trial no instructor objects to at least one section of the low students.

H. W. MILLER

UNIVERSITY OF MICHIGAN

## ON VALONIA AND HALICYSTIS IN EASTERN AMERICA*

THE use of large coenocytic algae in the study of protoplasmic permeability and the accumulation of salts in the vacuolar sap has proved so valuable that a note on certain distinctions with regard to them may be of interest. The best known species is *Valonia macrophysa* Kütz., of Bermuda, where it is widely distributed, but reaches large size only in favorable situations. It then occurs in branching clusters, which often are very tight and force the individual "cell" to grow into an elongated or tapering cylindroid. The separated "cell" is dark green and firmly turgid while alive and sinks in sea water. Its specific gravity is correlated with its sap content, as shown by Osterhout and Dorcas,[1] the solution of (chiefly) potassium chloride in the vacuole having a higher density than the sea water.

This cell contrasts markedly with the other Bermuda coenocyte studied, the one known locally as "sea bottles," washed ashore during part of the year on exposed beaches of the south shore. These are invariably single, pear-shaped cells up to an inch or more in diameter, and never clustered. These cells have not yet been found *in situ*, but when the stranded ones are carefully gathered many are found to live in the laboratory for a week or more. During this time they remain a pale to medium green in color, and have a distinct turgor, though the cell is much more resilient than that of *Valonia macrophysa.* Most marked of all, these cells continue to float in the sea water and only sink when the protoplast disorganizes and the green color disappears. This flotation during life is again correlated with the sap constitution (Osterhout and Dorcas, *loc. cit.*), the predominating salt being sodium chloride, and the solution slightly less dense than the sea water. Sulphate ion is excluded, and potassium is not markedly accumulated. On death, of course, all the salts of the sea water diffuse in, and the sap no longer remains light enough to float the protoplasm and cell wall.

These "sea bottles" of Bermuda have long been considered as *Valonia ventricosa*, a common species of the West Indies.[2,3] It was remarkable, of course, that two species of the same genus should differ so widely in fundamental character as to exercise such different selectivity with respect to salts. This distinction, together with differences in the cell wall, chloroplasts, distribution of nuclei and absence of lenticular or holdfast cells raised the suspicion that the floating form might not be *Valonia* at all, but *Halicystis,* a somewhat similar genus, known in the North Sea, the Mediterranean and the Pacific.[4] The occasional appearance of radiate and stellate markings quite similar to those figured by Kuckuck[5] in connection with zoospore formation in *Halicystis* strengthened this suspicion.

Further evidence was afforded the writer this summer, during a stay at the Carnegie Institution's marine laboratory at Tortugas, Florida. On the reefs surrounding those keys the cells of *Valonia ventricosa* J. Ag., are found *in situ,* and may be gathered for laboratory study. These cells are, again, always single, never in clusters or branching, and are usually almost spherical. They are, however, but slightly resilient, are dark green in color, and immediately sink in sea water, floating neither in the ocean nor while kept in sea water in the laboratory. The electrical conductivity of the sap indicates it to be chiefly potassium and not sodium chloride solution and is almost the same as that of *Valonia macrophysa,* gathered at Fort Jefferson, in Tortugas. Chemical analysis will be reported elsewhere in detail.

It is evident that this Florida species, morphologically similar to the *Valonia ventricosa* widely distributed in the West Indies, is something quite distinct from the Bermuda "sea bottle." It is therefore gratifying to report that specimens of the latter (and of one of three cells found floating at Tortugas, closely resembling the Bermuda form) were submitted to Dr. Marshall A. Howe, of the New York Botanical Garden. Dr. Howe has very kindly identified the floating cells as being indeed *Halicystis.* The species remains in doubt, *H. ovalis* (Lyngb.) Aresch. being evidently a smaller plant.

The conclusions we may draw are: (1) Physiological differences may be useful in taxonomic diagnosis,

* Contributions from the Bermuda Biological Station for Research. No. 155.

[1] Osterhout, W. J. V., and Dorcas, M. J., *J. Gen. Physiol.*, 1924–25, vii, 633.

[2] Collins, F. S., and Hervey, A. B., *Proc. Am. Acad. Arts and Sc.*, 1917, liii, 51.

[3] Britton, N. L., "Flora of Bermuda," New York, 1918, p. 494.

[4] Setchell, W. A., and Gardner, N. L., *Univ. California Pub., Botany,* 1920, viii, 154–5.

[5] Kuckuck, P., *Bot. Ztg.*, 1907, lxv, 139.

especially in calling attention to suspected differences; (2) within the genus *Valonia* these differences are not as great as at first indicated, and the genus has greater unity; (3) *Halicystis* is probably not closely related to *Valonia,* but may come of a widely differing ancestral stock, one in which sodium instead of potassium is selectively accumulated. This would further justify placing the genus in another family, the Protosiphonaceae.[6, 7]

It is also evident that another genus of these large "cells," new to our Atlantic coast and adapted to direct studies in permeability, will on account of its variation from the others, further increase the comparative value of such studies.

L. R. BLINKS

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## SOCIETIES AND ACADEMIES

### THE CORDILLERAN SECTION OF THE GEOLOGICAL SOCIETY OF AMERICA

THE Cordilleran Section of the Geological Society of America, which includes all members of the society resident in the Pacific Coast and Rocky Mountain States, held its twenty-sixth annual meeting on January 28 and 29, 1927, at the University of California in Los Angeles. William J. Miller, chairman of the section, presided. The Pacific Coast Branch of the Paleontological Society of America also met on January 29. The annual dinner was given under the auspices of the Branner Geological Club.

The program, comprising some thirty papers, was varied and interesting. The majority of the papers concern the fields of structural and physiographic geology. Representative titles were: "Origin of the Desert Basins of Southwest United States," by Eliot Blackwelder; "The Dissected Fault Scarp of the Wasatch Range, Utah," by William Morris Davis; "Origin of Normal Faults," by Chester W. Washburne; "Three Important Relief Sustaining Formations of the Southwest," by Robert T. Hill; "Current Geological Investigations in Japan," by T. Wayland Vaughan; "Experiments in Shear-Zone Folding," by Howard W. Kitson; "The Physical Indications of the Bridge Creek Flora," by Ralph W. Chaney; "The Camels of the McKittrick Pleistocene," by John C. Merriam and Chester Stock.

Approximately fourteen fellows of the Geological Society and seventy-five guests, mainly geologists, attended the meetings.

[6] West, G. S., "Algae," Cambridge, 1916, vol. i, pp. 223–4.

[7] Blackman, F. F., and Tansley, A. G., *New Phytologist,* 1902, i, 115.

As an incident probably without precedent in geological meetings in this country, a slight but very distinct earthquake disturbed somewhat the presentation of a paper concerning active faulting. The shock occurred in the midst of a discussion by James P. Fox, of the University of California, of the active Haywards Rift and other recent faults of Central California. It seemed that Nature was protesting, by peculiarly appropriate means, against the investigation and discussion by Inquisitive Man, of one of her normal processes.

JOHN P. BUWALDA,
*Secretary.*

### THE UTAH ACADEMY OF SCIENCES

THE twentieth annual meeting of the Utah Academy of Sciences convened at Hotel Newhouse, Salt Lake City, April 8 and 9. The program consisting of twenty-one papers was roughly divided into three sessions, one on Friday evening comprising six papers on research in education, and two sessions on Saturday. The forenoon session was devoted to papers on natural science including agriculture, agronomy and bacteriology. In the afternoon papers on subjects in the physical sciences were read. There were a total of twenty-two papers in all, the heaviest program the academy has had for a number of years.

The sessions were all well attended and the interest was good throughout. The attendance from the education people at the Saturday sessions, however, was noticeably small.

At the business session following the program of papers the following officers were elected for the ensuing year:

President, R. A. Hart, Salt Lake City.

First Vice-President, Dr. Jos. F. Merrill, University of Utah.

Second Vice-President, Dr. Willard Gardner, Utah Agricultural College.

Council: Dr. Bert L. Richards, Utah Agricultural College, Professor O. W. Israelson, Utah Agricultural College, Dr. Vasco M. Tanner, Brigham Young University.

Secretary: Mr. J. Cecil Alter, Salt Lake City.

Twelve new members were added to the academy and five members were advanced to fellowship.

This was the first meeting in many years to be held down town, the previous meetings having been held at the university. The experiment seemed to be successful both in the number attending the different sessions and in the interest shown. The newspaper publicity was better than we usually get, undoubtedly due to the convenience of getting reporters to the meetings.

C. ARTHUR SMITH,
*Secretary.*